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LATEST AND MOST IMPORTANT DISCOVERIES

IN

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Inflammation of the Breast and Milk Abscess. By Thomas William Nunn, Surgeon to the Western Dispensary, Demonstrator of Anatomy at the Middlesex Hospital, London, 1853. Post 8vo. Pp. 52.

The Treatment of Obstinate Ulcers and Cutaneous Eruptions on the Leg without Confinement. By Henry T. Chapman, F.R.C.S., late Surgeon to the St George's and St James's Dispensary, and sometime Lecturer on Surgery at the School of Medicine adjoining St George's Hospital. Second Edition. London, 1853. 12mo. Pp. 156.

On Continuous Molecular Changes, more particularly in their relation to Epidemic Diseases; being the Oration delivered at the 80th Anniversary of the Medical Society of London. By John Snow, M.D., Vice-President of the Society. Published by request of the Society. London, 1853. 8vo. Pp. 38.

The Sophistry of Empiricism. London, 1853. 8vo. Pp. 84.

The Medication of the Larynx and Trachea. By S. Scott Alison, M.D., Member of the Royal College of Physicians, London. London, 1853. Post 8vo. Pp. 49.

Homœopathy: its Tenets and Tendencies, Theoretical, Theological, and Therapeutical. By James Young Simpson, M.D., F.R.S.E., Professor of Midwifery, University of Edinburgh, &c. &c. Third Edition. Edinburgh, 1853. 8vo. Pp. 286.

On a New Method of Managing Fractures; from the Address on Surgery delivered at the 20th Anniversary Meeting of the Provincial Medical and Surgical Association held at Oxford on Wednesday and Thursday, July 21st and 22d 1852. By James Torry Hester, Fellow of the Royal College of Surgeons; Senior Surgeon to the Radcliffe Infirmary, Oxford, &c. London, Oxford, Worcester, 1853. 8vo. Pp. 11.

A Pathological and Practical Treatise on Epidemic Cholera: its History, Causes, various Forms, and Treatment. By O. R. Mahony, L.F.P. and S.L.S.A. London, 1853. Post 8vo. Pp. 184.

The Subject Matter of a Course of Six Lectures on the Non-Metallic Elements. By Professor Faraday; delivered before the Members of the Royal Institution in the Spring and Summer of 1852. Arranged by permission from the Lecturer's Notes sent for the occasion. By J. Scoffern, M.B., late Professor of Chemistry at the Aldersgate College of Medicine. To which is (are) appended Remarks on the Quality and Tendency of Chemical Philosophy, on Allotropism, and Ozone; together with manipulative details relating to the performance of experiments indicated by Professor Faraday. London, 1853. 12mo. Pp. 293.

On the Use of an Artificial Membrana Tympani in cases of Deafness, dependent upon Perforation or Destruction of the Natural Organ. To which is added a Paper entitled, Ought the Tonsils or Uvula to be excised in the Treatment of Deafness? By Joseph Joynbee, F.R.S., Fellow of the Royal College of Surgeons of England; Aural Surgeon to St Mary's Hospital; Consulting Aural Surgeon to the Asylum for the Deaf and Dumb, &c. &c. London, 1853. 8vo. Pp. 46.

The Harmonies of Physical Science in Relation to the Higher Sentiments; with Observations on the Study of Medical Science and the Moral and Scientific Relations of Medical Life. By William Hinds, M.D. London, 1853. 12mo. Pp. 195.

Change of Climate considered as a Remedy in Dyspeptic, Pulmonary, and other Chronic Affections; with an account of the most eligible Places of Residence for Invalids in Spain, Portugal, Algeria, &c., at different Seasons of the Year, and an Appendix on the Mineral Springs of the Pyrenees, Vichy, and Aix les Bains. By D. J. T. Francis, M.D., London, &c. London, 1853. Post 8vo. Pp. 339.

On the Nature and Treatment of the Deformities of the Human Frame; being a Course of Lectures delivered at the Royal Orthopædic Hospital in 1843; with Numerous Notes and Additions to the Present Time. By W. J. Little, M.D., Licentiate of the Royal College of Physicians; Lecturer on the Practice of Medicine at, and Physician to, the London Hospital, &c. &c. Illustrated by upwards of 160 Engravings and Diagrams. London, 1853. 8vo. Pp. 412.

Observations on the Nature and the Treatment of the Asiatic Cholera. By William Stevens, M.D., D.C.L., Oxon. London, 1853. London, New York, Paris, Madrid, 1853. 8vo. Pp. 499.

A Treatise on Diseases of the Heart. By O'B. Bellingham, M.D., F.R.C.S., Member of the Court of Examiners of the Royal College of Surgeons in Ireland, one of the Medical Officers of St Vincent's Hospital, &c. Dublin, London, Edinburgh, 1853. 8vo. Part I. Pp. 252.

The Progress of Improvement in the Treatment of Consumption, and other Pulmonary and Laryngeal Diseases, and on some New Remedial Means. By James Turnbull, M.D., Physician to the Royal Infirmary, and Lecturer on Clinical Medicine in the Medical School. London, 1853. 8vo. Pp. 74.

On the Nature and Proximate Cause of Insanity. By James George Davey, M.D., formerly of the Middlesex Lunatic Asylums at Hanwell and Colney Hatch, &c. &c. London, 1853. Post 8vo. Pp. 76.

On the Pathology and Treatment of Acute Rheumatism. Being the Lumleian Lectures delivered before the Royal College of Physicians in 1853. By James Alderson, M.D., F.R.S.; Senior Physician to St Mary's Hospital, and formerly Fellow of Pembroke College, Cambridge. London, 1853. 8vo. Pp. 107.

Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England. By James Paget, F.R.S., lately Professor of Anatomy and Surgery to the College; Assistant Surgeon and Lecturer on Physiology at St Bartholomew's Hospital. Volume I. 8vo. Pp. 499. Volume II. 8vo. Pp. 637. London, 1853.

Chemistry of the Four Seasons,—Spring, Summer, Autumn, Winter; An Essay concerning Natural Phenomena admitting of interpretation by Chemical Science, &c. By Thomas Griffiths, late Professor of Chemistry in the Medical College St Bartholomew's Hospital, &c. &c. London, 1853. 12mo.

On the Decline of Life, in Health and Disease. Being an Attempt to Investigate the Causes of Longevity, and the best means of attaining a Healthful Old Age. By Bernard Van Oven, M.D., Fellow of the Royal Medical and Chirurgical Society, &c. &c. London, 1853. 8vo. Pp. 300.

Observations on the Structure and Development of Bone. By John Tomes, F.R.S., Surgeon-Dentist to the Middlesex Hospital; and Campbell de Morgan, Surgeon to the Middlesex Hospital. 4to. Pp. 30. (Transactions of the Royal Society.)

Concours pour l'Aggregation (Section de Medecine.) De l'Etat Actuel des Connoisances acquises en Hematologie, et des Consequences Pratiques qui en de coulent. These presentée et soutenue par J. D. Tholozan, Professor Agregé a l'Ecole de Medecine et de Pharmacie Militaires. A Paris, 1853. 4to. Pp. 112.

British and Foreign Medical Review, or Quarterly Journal of Practical Medicine, No. xxii. April 1853. London.

The Dublin Quarterly Journal of Medical Science. No. xxv. May 1853. Dublin.

The American Journal of the Medical Sciences. Edited by Isaac Hays, M.D.L. No. L. New Series. April 1853. Philadelphia.

The Retrospect of Medicine: A Half-Yearly Journal containing a Retrospective View of every Discovery and Principal Improvement in the Medical Sciences. Edited by W. Braithwaite. Vol. XXVII. January—June 1853.

Weekly Returns of Births and Deaths in London, by Authority of the Registrar-General. No. 13, Week ending Saturday 26th March, to No. 26, Week ending Saturday June 26, 1853.

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———— page 201, fourth line from foot, *for* Savanna, *read* Surinam.

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr Stratton and Dr Cottle.

The following Works have been received:—

Smallpox and Vaccination. Return to an Order of the Honourable the House of Commons, dated 26th April 1853;—for Copy of Letter from Dr Edward Seaton to Lord Palmerston, with inclosed Copy of a Report on the state of Smallpox and Vaccination in England and Wales, and other countries, and on Compulsory Vaccination, with Tables and Appendices, presented to the President and Council of the Epidemiological Society, by the Smallpox and Vaccination Committee, the 26th day of March 1853. Whitehall, 2d May 1853. Ordered by the House of Commons to be printed, 3d May 1853.

Report on the Cholera in Jamaica, and on the General Sanitary Condition and Wants of the Island. London, 1853. Folio. Pp. 133.

The Medical Committee of the Charing-Cross Hospital Medical School, with the defects of that Institution. By Edward Smith M.D., LL.B., B.A., London. Fellow (by Exam.) of the Royal College of Surgeons, England, Lecturer on Anatomy at the Charing-Cross Hospital Medical School, &c. London, 1853. 8vo. Pp. 36.

Sketch of the Operation, and of some of the most striking results, of Quarantine in British Ports, since the beginning of the present century. By Gavin Milroy, M.D., Fellow of the Royal College of Physicians, Royal Medical and Chirurgical Society, &c. London, 1853. Post 8vo. Pp. 38.

On the Prevalent Treatment of Disease two Lectures delivered in the Theatre of the Royal College of Surgeons in July 1853. By Frederic C. Skey, F.R.S., Professor of Anatomy and Surgery to the College, &c. &c. London, 1853. 8vo. Pp. 68.

Practical Observations on Aural Surgery, and the Nature and Treatment of Diseases of the Ear with illustrations. By William R. Wilde, Fellow of the Royal College of Surgeons in Ireland, Surgeon to St Mark's Ophthalmic Hospital, &c. &c. London, 1853. 8vo. Pp. 506.

On the Physiological Uses of the Ganglionic Nervous System. By James George Davey, M.D., F.R.S.A., &c. &c., lately Medical Superintendent of the County of Middlesex Lunatic Asylums at Hanwell and Colney Hatch (Fem. Dep.), and formerly of the Civil Medical Service of Ceylon (E. I.) London, 1853. 8vo. Pp. 20.

1. On Animal and Vegetable Fibre as originally composed of Twin Spiral Filaments in which every other structure has its origin; a Note, &c. By Martin Barry, M.D., F.R.S., F.R.S.E.

2. On the Penetration of Spermatozoa into the interior of the Ovum; a Note, &c. By M. Barry, M.D., &c.

3. Researches in Embryology; a Note Supplementary, &c. By M. Barry, M.D., &c. 8vo. Pp. 6.

(Abstract of Proceedings of the Royal Society, 17th March 1853, and 16th June 1853.)

A Practical Treatise on Sea-Bathing and Sea-Air. By George Hartwig, M.D. London, 1853. (Highley's Popular Medical Series, No. 1.) 12mo. Pp. 148.

Sandgate as a Residence for Invalids. By George Moseley, Member of the Royal College of Surgeons of England; Licentiate of the Society of Apothecaries; Fellow of the Medical Society of London, &c.; and Surgeon to the Sandgate Dispensary, London. Sandgate, 1853. 12mo. Pp. 136.

Elements of Psychological Medicine: an Introduction to the Practical Study of Insanity. Adapted for Students and Junior Practitioners. By Daniel Noble, F.R.C.S., Medical Officer to the Clifton Hall Retreat; and Lecturer on Psychological Medicine at the Chatham Street School of Medicine, Manchester. London, 1853. 12mo. Pp. 340.

The British and Foreign Medical Review or Quarterly Journal of Practical Medicine, No. XXIII. July 1853. London.

The Dublin Quarterly Journal of Medical Science, No. XXXI. August 1853. Dublin.

Weekly Returns of Births and Deaths in London. By authority of the Registrar-General. No. 27, Week ending Saturday, 2d July 1853, to No. 38, Week ending Saturday, 17th September 1853.

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EDINBURGH
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1st JULY 1853.

PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*Upon the Influence of the Nervous System on the Movements of the Iris.* By Professor Julius Budge of Bonn. From the German. [Aus dem Archiv fur Physiologische Heilkunde; Ergänzungsheft, 1852.] From the Archives of Physiological Medicine. Supplementary Number, 1852.

As the eye is named the Mirror of the Soul, so, and probably with still greater justice, may it be named the Mirror of the Body. Scarcely is there any other part in which the most important functions of the animal organism are reflected in so definite and distinct a manner in all their stages, as directly in the eye. Further, in this organ, again, is a part which, like the hand of the watch, gives knowledge of the internal life of this remarkable organ. This hand or index is the pupil, and the dial-plate is the Iris. In this small disk is deposited a quantity of energetic forces, which are called into action partly by the blood, partly by the nerves, and are accomplished by the muscles of the Iris.

The present inquiry is to shew, neither the influence of the blood upon the action of the Iris, nor the relation of the Muscles of this organ to their subject, but simply the energy of the individual nerves of the Iris. Or, in other words, the object of this Essay is, so far as is possible, to shew by what conditions in the nervous system, the determination which the Iris receives, is accomplished.

By means of the movements of the Iris, the pupil is wont normally to be contracted and dilated, according as a less or greater degree of light is required for the eye. The distinctness of vision depends essentially on this circumstance, that the luminous rays are concentrated in as great a degree as possible, in the centre of the lens, and fall in as small a degree as possible upon the edges of the lens. The more the lens is covered at the edges, that is, the smaller is the pupil, so much more distinct, *cœteris paribus*, is vision, Hence, the light ought naturally to be sufficiently intense, in order to produce within a small space a sufficient degree of illumination. With the diminution of the light, the necessity increases to produce more room for the admission of the luminous rays ; that is, the necessity to dilate the pupil takes place.

A second function the Iris shares also in common with other muscles. In all muscles, namely, there is a sensation to distinguish the condition itself ; that is, their relaxation, diminution of their energy, weariness, the degree of contraction, and other circumstances. The Iris also possesses this sensation, and here also it has the like object.

In order to investigate the nervous apparatus of the Iris, and its mode of action, I have, partly in conjunction with Dr Waller, partly by myself, performed a series of experiments, which have thrown additional light upon this subject. In order to prevent frequent repetitions, I here observe, that in the description of the experiments common to both, the plural number is used ; in those performed by me alone, the singular number is employed.

In the Iris are distributed three nerves ; namely, one branch from the third cerebral nerve, the *Nervus Oculo-motorius ;* one branch from the fifth cerebral nerve, that is, from the *Nervus Naso-ciliaris*, the first twig of the Trigeminal Nerve ; and lastly, the branches of the Sympathetic Nerve. To the last belong those filaments which, after they have entered with the internal carotid artery into the cavity of the Cranium, appear to go to the ganglion of Gasserius, and to coalesce with it. The Naso-ciliar Nerve, and the Oculo-muscular Nerve, give branches to the Ciliary Ganglia, and form those filaments which are called the Roots of this Ganglion. The Naso-ciliar Nerve conveys to the long root of this ganglion (the Ciliary), filaments from the Trigeminal Nerve and from the Sympathetic Nerve. Through the short root of the Ciliary Ganglion, proceeding from the Oculo-muscular Nerve, the Iris receives those nervous filaments which cause the pupil to contract; through the long root it receives, in the first place, filaments which serve for the dilatation of the pupil, and these belong to the Sympathetic Nerve; and also, in the second place, the sensational filaments passing from the Trigeminal Nerve, and partly motific filaments. Consequently the *Nervus Oculo-motorius*, serves for causing the contraction of the pupil; the Sympathetic Nerve for its dilatation; and the Trigeminal Nerve is the essential sensific Nerve of the Iris. *See* § III.

§ I. CONTRACTION OF THE PUPIL BY MEANS OF THE *Nervus Oculo-motorius*.

Contraction of the Pupil is effected by the contraction of the circular bands or ligaments which are situate in the Iris. These fibres are not present in the same amount in all animals; and by this circumstance, probably, is regulated the force and the facility with which contraction of the pupil is effected. In BIRDS, the circular fibres of the Iris have the preponderance in breadth over the other fibres, so that it is in general still doubtful whether others as annular fibres exist. Among the MAMMALIA, this species of fibres appears to be more abundant in Dogs and Cats than among Rabbits; at all events, upon the application of irritations to the Oculo-muscular Nerve in the former orders of animals, the contraction of the pupil takes place much more quickly and forcibly than in the latter orders of animals.

In order to ascertain the mode and degree of operation of the Oculo-muscular Nerve, we have, in a great number of animals,—especially in Rabbits, Dogs, Cats, Pigeons, Ducks, and common Fowls,—applied irritations to these Nerves, partly in living animals which were previously deprived of sensibility by means of Ether, and also after decapitation. On the 12th of December 1851, a murderer suffered death by the guillotine in this city of Bonn; and we availed ourselves of this opportunity to observe the phenomena in the human subject. The irritability of the Oculo-muscular Nerve remained in this instance entire after the lapse of twenty minutes,—a longer time than is the case in many animals.

When in animals still alive the experimenter exposes the nerve now mentioned, in order to apply to it any irritation, it is always the best course to remove the skull-cap. Because, though it is possible, by means of a sharp instrument which he causes to penetrate into that spot of the skull which corresponds to the seat of this nerve, to reach the exact point, yet this is still an uncertain manœuvre. The operator is never certain whether he has encountered the nerve principally, nor whether he has touched that nerve alone, and no other part. Besides, mechanical irritation is never so certain as galvanic irritation.

The removal of the skull-cap by means of a pair of accurately-cutting nippers is, even in animals not stupified by ether, an operation attended with little pain, if the operator does not go too far backward. and avoids coming too near the eyeball. By means of stupefaction by ether, however, the pain is entirely removed.

On the other hand, the hæmorrhage certainly is often considerable and troublesome; yet it seldom proceeds to such an extreme as to destroy the animal. When the brain is exposed, the operator may either, after dividing the Dura Mater, raise anteriorly the whole half-hemisphere as far as the base, which, when it is cautiously done, is effected without pain;—or he may cut through the olfactory and optic nerves, in order to reach the Oculo-muscular Nerve from before. In this last part of the experiment nothing causes so much hindrance as the hæmorrhage, which naturally cannot be prevented, and which the operator endeavours to abate by the employment of cold water and the application of the sponge.

On the other hand, the raising of the Brain from before is effected entirely without pain; and we have at no time seen that an irritating cause applied to the Oculo-muscular Nerve was connected with pain, although we have well and sufficiently observed this. This phenomenon comes so much more directly before the eyes, that the Trigeminal Nerve, which lies close to it, evinces an extreme degree of sensibility. The slightest touch of this nerve produces immediately violent reaction. There are, indeed, some inquirers,—as Muller, Magendie, Valentin,—who ascribe sensibility to the Oculo-muscular Nerve; our experiments, however, fully agree with the results given by M. Longet, who has observed in the Oculo-muscular Nerve the same absence of sensibility which we have seen. The close vicinity of the Ophthalmic branch of the Fifth Pair may render very difficult the discrimination in this case. M. Valentin has, by means of the Neurotome, undertaken the division of the Oculo-muscular Nerve, and in this operation he observed indications of pain. It is my belief, that in this instance likewise, the Fifth Pair, which is situate close by the Third, was touched; and that in this manner the pain arose. We should certainly once or twice, in detaching the Oculo-muscular Nerve from the Peduncle of the Brain, have observed some degree of movement in the animal, if this nerve had belonged to the class of sensiferous nerves.

On the other hand, the action which the Third Pair evinces as a motific nerve is shewn in a very determinate manner. It is well known that this nerve goes to the *Musculus Levator palpebræ superioris*, the *Rectus superior oculi*, the *Rectus inferior*, the *Internus*, and the *obliquus inferior*. All these muscles may be thrown into action by irritation of the nerve; but predominant is the turning of the eyeball inwards, produced by the *Rectus internus* Muscle. This inversion of the eyeball remains also longest after death, and, on the application of strong irritation, is the most remarkable. In the person, for example, who suffered decapitation, in whom the Oculo-muscular Nerve was irritated about ten minutes after beheading, we observed no other action of the muscles of the eyeball except a forcible rolling inwards. This, however, is noticed only by the way.

The action which here comes principally under consideration, is the narrowing or contracting of the Pupil. As soon as the wire of a galvanic apparatus touches the Oculo-muscular Nerve (*Nervus Oculo-motorius*), the Pupil is contracted, and after the irritation is terminated, it is again dilated. The result follows most speedily in Birds; and it is hence convenient to perform the experiment first in Birds, and indeed forthwith after death. The experimenter, for example, beheads a duck or a pigeon, takes away the skull-cap, removes the brain, isolates the Oculo-muscular Nerve by means of a glass rod, and allows the galvanic current to pass through it. In one instant the Pupil becomes narrower by one-half, and more; and so soon as the experimenter removes the wire from the nerve, the Pupil is again restored to its previous diameter. This instantaneous result is entirely as we are in the habit of observing it in the muscles of the spine. We know that in the same instant, almost, in which the crural nerves of a frog are touched, convulsive movements take place in the muscles of the legs, and immediately terminate on the cessation of the irritating agent. M. E. Weber has in recent times carefully studied this rapidity in the contraction of the Iris in Birds, and gives this explanation of it, that the Iris in Birds has cross-striped muscular fibres; and it is a peculiarity of all cross-striped muscular fibres, with the exception of the muscles of the heart, to contract rapidly as soon as irritation is applied, while the cylindrical fibres neither begin to contract at once with the irritation, nor cease to contract after the irritation ceases. This phenomenon, however, as I shall prove, must have for its cause some other condition than that which depends on the structure of the muscles. Because, in the first place, nature makes no exception; and were it in truth alone the cross-striped muscles on which this phenomenon depends, the heart would act in the same manner. In the second place, the Iris of Mammiferous animals, in which the fibres are cylindrical, is different in its contractions when different nerves are irritated. In rabbits, the Pupil may be made to contract through the Oculo-muscular Nerve,[1] the Ophthalmic branch of the Trigeminal Nerve (Fifth Pair), and the *Medulla oblongata;* but the contraction, which ensues by virtue of the first of the three named nervous organs, takes place very quickly, and as quickly subsides. It is maintained subverted, though the experimenter irritates the two other nervous organs. Here a considerable time elapses until the phenomenon appears, and a space still longer until it ceases. I cannot, therefore, directly agree that the cross-striping of the muscles contributes to the rapidity of the contraction; and I think that some other circumstance comes into consideration, which in like manner co-operates; and this other circumstance, to all appearance, must be sought for in the Nerve itself.

In the same manner as in birds, does the Pupil undergo contraction after irritation of the Oculo-muscular Nerve (*Nervus Oculo-motorius*), in MAMMALIA, among which the subjects of our experiments were Rabbits, Cats, and Dogs. If the animal remains in life during the experiment, the action in no case is wanting. But likewise, when the head has been cut off, the experimenter may be convinced of the fact with the same certainty. Naturally, however, this is only possible so long as the irritability of the nerve continues. When this irritability is exhausted, the operator observes no result. To every one who has experimented much, it is a well-known fact how differently Irritability displays itself in different genera, species, and in different individuals in the same species; how the experimenter may prolong in the one animal for half an hour, for a whole hour, after death, and longer, without disturbance, an experiment which in another cannot be continued beyond fifteen minutes, and not so long. Of the circumstances on which these variations depend, so little is known scientifically, that no general rules hitherto can be established. Of great influence in this respect is good feeding, and good care of the animals in general. When the animals have not sufficient and suitable food, when they have not sufficient room in their stalls and kennels, if these are not kept clean, it is very general that the irritability is quickly exhausted after death. But besides these demonstrable causes, there are many others, unknown, which induce similar modifications. So certain is it, that this difference is very frequently the cause why an inquirer has made observations, correctly and accurately indeed at the time, yet which he himself and others either could not again perceive, or perceived very imperfectly. Nay, while an animal is still in life, while it continues to breathe, and its sensibility continues to act, irritability may in individual parts decline and disappear. The opening of the vertebral canal may distinctly impair the irritability of the spinal marrow.

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[1] It may here be observed that the author uses throughout the name of *Nervus Oculo-motorius*, given by Soemmerring to designate the third pair of Cerebral Nerves, or what is named sometimes the Oculo-muscular Nerve, from its supplying all the muscles of the eye excepting the superior oblique (M. Trochlearis).

Not unfrequently even it occurs, that when we irritate after death, any nerve which sends branches into different parts, one part may yet give rise to movements, and another no longer does so. This I might demonstrate from different muscles of the leg of the frog; but in order not to depart too far from my subject, I shall merely recal what I have already above mentioned. If, for instance, we irritate a certain time after death, the whole oculo-muscular nerve (*Nervus Oculo-motorius,*) the eyeball performs only a certain turning inwards, no other movement. For the most part, even the Irritability for this movement and its muscle continues longer than that of the Iris ; and we have repeatedly seen that the pupil did not in the smallest degree undergo further contraction, while the eyeball itself continued very distinctly to roll inwards.

It is therefore not striking, though inexplicable, that,—in some instances, immediately after the death of the animal, when we open the skull, and irritate the oculo-muscular nerve (*Nervus Oculo-motorius*),—we still no longer observe contraction of the pupil. We ought, therefore, neither to be satisfied with performing only some few experiments, nor should we neglect to make the proper investigation during the life of the animals. In rabbits, the irritability appears to depart most rapidly ; yet here there are many exceptions. I know not whether it were accidental, that exactly the experiments on three dead rabbits furnished the most striking and the most constant results, in which the experiment was performed in clear sunshine, and after division of the optic nerves. In dogs and cats, irritability often continues half an hour after death in the oculo-muscular nerve, and often it is very quickly extinguished.

In the case of the head of the person who suffered death by decapitation, the phenomenon was in an extraordinary degree clear and conclusive. Scarcely had the wire of the apparatus touched the nerve, when the pupil began to be contracted, and in a short time it was as small as the head of a pin. The difference between the irritated and the unirritated side was in the highest degree striking, so that even persons not belonging to the profession, who were present during the experiment, recognised at the first glance the powerful influence.

The experiment which MM. Duval, Rochard, and Petit, performed on the 26th of March 1851, upon a man who was beheaded, furnished the same results. "Between twelve and thirteen minutes after the head had fallen under the knife of the guillotine, irritation was applied to the oculo-muscular nerve; (*N. Oculo-motorius*). The actions were very immediate. The pupil, which had been greatly dilated, contracted immediately to one-half. This contraction, which was at least as speedy as that produced by the influence of light, proceeded speedily to dilatation as soon as the irritation was ended."

"We remark by the way, that in these instances also, as in other instances which we have previously observed, the pupil shortly after death was greatly dilated; two days afterwards, it had attained its greatest contraction."[1]

In rabbits and frogs I have observed the same phenomenon. Thus, for instance, the diameter of both eyes (pupils) of a rabbit still in life, measured, without any operation having been performed on the animal, eight millimetres, and then twenty-four hours after death, only six millimetres.

In short, when the conditions are the same, the consequence is always everywhere the same.

This fact, that the contraction of the pupil may be produced by the third pair of nerves, has, nevertheless, by no means been discovered by us, but has been known for a long time. In the writings of Fowler, Reinhold, Nysten, Herbert Mayo, Longet, and other authors, we find the third pair distinguished as that by which the contraction of the pupil is accomplished; and this point would not need to be made the subject of inquiry so fully, were it not that recently doubts have been raised regarding the correctness of the fact.

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[1] Gazette Medicale, 1851, p. 437.

It is the belief of MM. Volkmann and E. Weber, that, by means of their experiments on irritation, they have obtained the result, that the oculo-muscular nerve, (*Nervus Oculo-motorius*) does not always act in the same manner; that it is capable of producing equally contraction and dilatation of the Pupil. When M. Volkmann applied irritation to this nerve in a cat and in rabbits, he saw that the Pupil became dilated, and when he performed the same experiment on two dogs and on a horse, contraction of the Pupil followed. In the head of a rabbit which had been decapitated, M. E. Weber, in like manner, saw dilatation of the pupil take place when the operator touched with the end of the conducting wire the oculo-muscular nerve within the cavity of the skull. In consequence of these observations, M. E. H. Weber, in an essay which appeared last year, *De Iridis Motu*, represents the action of the *Nervus Oculo-motorius* upon the Iris with greater definitiveness. The passage here referred to is found at page 104, and is the following:—

"Valentin observed in rabbits, that the pupil is most vehemently contracted at that instant at which the oculo-muscular nerve was divided within the skull. But since it is now agreed that the Pupil in Mammiferous animals, when the oculo-muscular nerve is irritated by galvano-magnetic shocks, is not contracted, but dilated, the experiments of Valentin ought to be cautiously repeated, in order that we may obtain certain information on a matter so subtile and difficult."[1]

In these observations, nevertheless, there is distinctly contained an inherent improbability. Because it is not in nature that any nerve can shew itself in a manner entirely opposite at different times.

Our experiments, however, have afforded the explanation of this apparent contradiction. This is, that the galvanic irritations of these inquirers were applied, without the *Nervus Oculo-motorius* having been previously isolated by means of a glass rod placed beneath it; and it is possible that the shock might be communicated, and in parts lying closely together there might be produced an irritation, in which the *Nervus Oculo-motorius* has no concern. So long as the irritability of this nerve is continued after death, the slightest touch with the wire is distinctly sufficient to throw it into action;

[1] "Valentinus [De Functionibus Nervorum, p. 18] in cuniculis observavit, pupillam eo momento vehementissime contractam esse, quo nervus oculorum motorius in cranio discindebatur. Cum vero nunc constet, pupillam Mammalium, nervo oculorum motorio ictibus galvano-magneticis irritato non contrahi, sed dilatari, pericula Valentini caute iteranda sunt, ut de re tam subtili et difficili certiores fiamus."

while the nerve which produces dilatation of the pupil, namely, the sympathetic nerve, requires a more lengthened application of the irritating agent. After death, the oculomuscular nerve dies first; in the sympathetic nerve, life ceases at a much later period. It is thus very easily explained why the operator, by applying irritation to the *Nervus Oculo-motorius*, believes that he produces in some instances contraction of the pupil, in other instances dilatation of that aperture. If the operator, after the death of the animal, has removed the half or the whole of the brain, and if he applies the irritation to the nerve, close to its origin, so that between the nerve and the bone there is situate a stratum of atmospherical air, the operator witnesses either contraction of the pupil, or no action whatever. If, on the other hand, he applies the irritation to the nerve upon the bone, there ensues at first, that is, so long as the nerve continues irritable, Contraction of the pupil; afterwards, by the agency of the sympathetic nerve, Dilation. At this time, if the operator only places the *Nervus Oculo-motorius* upon a bit of glass, he observes in no case dilatation, and, in general, no action whatever. If he withdraws the glass, forthwith dilatation is manifest when irritation is applied. Thus it is in dogs and rabbits. In the head of the person who suffered death by the guillotine, we made use of no glass for the purpose of isolating the nerve; contraction of the pupil was manifested as late as twenty minutes after decapitation; afterwards, dilatation was observed.

There is, however, still another mode in which it is possible to shew whether actually the *Nervus Oçulo-motorius* possesses the peculiarity of inducing contraction in the Pupil, and also dilatation or not. This is truly, that after dividing a motific nerve, its peripheral end,—that is, the portion which has no connection with the Centre of the Nervous System,—loses its irritability. Particularly it is known from the experiments of M. Longet, that generally after a few days, and regularly on the fourth day after the section of a nerve, the peripheral end has lost its irritability, so that even the most delicate branches no longer manifest any action, while the muscles continue still irritable after the lapse of many weeks. These observations have been confirmed by other inquirers.

In the sympathetic nerve, also, a similar disposition is observed. Already six days after it has been divided, its irritability is extinguished. We extirpated in several rabbits the sympathetic nerve in the neck, and at the same time its superior ganglion, and waited for one or more weeks, before we put the animals to death. At first, we in every instance convinced ourselves that the sympathetic nerve on the right side of the neck, where it had been divided, no longer gave evidence of reaction. When the skull was broken up immediately after death in a rabbit treated in this manner, we observed, by applying galvanic irritation to the *Nervus Oculo-motorius*, no result, although we had applied the conducting wire to it, without isolating the nerve by means of glass placed beneath it. When we, on the other hand, applied the irritation in the same manner to the other nerve on the sound side, the pupil became dilated in the wonted manner. Here consequently was given the unquestionable proof, that it is not the oculo-muscular nerve the irritation of which produces the effect of dilating the Pupil; and that this result depends rather upon the sympathetic nerve, upon which the irritating agency proceeded.

The conclusion consequently follows, that no doubt can be entertained that by the agency of the *Nervus Oculo-motorius*, contraction of the pupil may be produced, but never dilatation of this aperture.

The next subject consequently requiring to be treated, is to discover the operations by means of which, during life, the oculo-muscular nerve is thrown into action; that is, the means by which the pupil is made to contract.

a. The usual cause of the contraction of the pupil is acknowledged to be the incidence of the light upon the Retina. It may be proved experimentally, that this phenomenon is accomplished by the agency of the Optic Nerve upon the Oculo-muscular Nerve. The light acts as an irritant to the optic nerve, and one effect of this irritation is the action of the oculo-muscular nerve, by means of which the pupil becomes contracted. Light acts not directly upon the Iris, as the experiments of Lambert, Muller, Fontana, and E. H. Weber,[1] have proved. Because, when by means of suitable contrivance the light is prevented from reaching the Retina, yet falls upon the Iris, the pupil remains unchanged. As soon, however, as the Retina is illuminated, the well known action instantly follows.

The excitement which light exercises upon the nervous filaments of the Retina and the Optic Nerve, may also be represented by other means, as already Mr Herbert Mayo and others have observed, and as I myself have confirmed. When a needle touches the Retina, the pupil undergoes contraction. If to the optic nerve irritation is applied within the cavity of the skull, the same action immediately takes place. This I have repeatedly seen in rabbits, which had been previously deprived of sensibility by means of ether, and in which, after opening the skull, the optic nerves were exposed and stimulated by galvanic agency. In every instance, when these nerves came in contact with the poles, the pupils underwent contraction. When the experimenter applied irritation only to one optic nerve, both pupils, nevertheless, underwent contraction; exactly as the pupil of one eye is known to be contracted when the light falls only upon the other eye.

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[1] Compare his Annalen Anatom. und Physiologischen, Fascicul. iii., p. 108.

The Optic Nerve gives off no branches which proceed to the Iris. The consequence of irritation applied to the optic nerve, consequently, cannot proceed from any direct operation. With this, also, accords the observation made by Dr Herbert Mayo. This is, that when the optic nerve has been divided, and the operator immediately afterwards applies irritation to the end which is no longer in connection with the brain, this has no effect in changing the diameter of the pupil.

From this, accordingly, it follows that the nervous conduction in the Optic Nerve is directed not towards the periphery, but towards the Centre; that the optic nerve induces the action of the Iris not directly. Another experiment performed by Herbert Mayo, and which I have also repeated, proves on the other hand that it is the *Nervus Oculo-motorius* which directly causes the action of the Iris. That is, providing the optic nerve remains uninjured, but the oculo-muscular nerve is divided, the action upon the pupil continues as before. This experiment the operator most certainly performs in the Mammalia, providing he opens the cranium after inducing insensibility by means of ether, divides the two olfactory nerves, one optic nerve, and then on the side on which he has divided the optic nerve, elevates the hemisphere of this side, and first detaches the one oculo-muscular nerve and then the other. The operator may, however, cause the pupil to contract, if he irritate the central end of the optic nerve after it has been divided.

Between the optic nerve and the oculo-muscular nerve, consequently, there exists a relation of the kind called reflected, as between the posterior and the anterior roots of the spinal marrow; that is to say, an energy is excited in the second, by means of another energy which has been excited in the first. But the excitation, and the consequent arousing of this excitation, is avowedly only possible, so long as the connecting member of the spinal marrow is between the two roots. It is not ascertained, whether the central nervous organ only maintains the conduction between the two roots, or whether a new force is produced in the same; and experiments require still to be performed upon this question, before we are in a condition to make a clear exposition upon this action.

It is also proved by recent experiments, that between the optic nerve and the oculo-muscular nerve, there is a central organ which acts exactly in the same manner as the spinal marrow to the anterior and posterior roots; this organ is the *Corpora Quadrigemina.* With this subject, Flourens, Hertwig, Magendie, Longet, and myself, have been particularly occupied, though in some points the statements made by these inquirers do not perfectly agree. This depends on the circumstance, that not the entire mass of the *Corpora Quadrigemina* was extirpated, and consequently not the full action took place which other observers beheld. When, for example, as already Flourens observed, only the upper part of the *Corpora Quadrigemina* is taken away, the influence upon the action of the Iris is transitory, as was also ascertained by Hertwig. On the other hand, the contraction of the Iris, in consequence of the influence of light, is entirely abolished when the *Corpora Quadrigemina* are completely cut away. M. Longet states, that after removal of the large hemispheres of the brain, as also the *optic Thalami*, the action of the Iris is not impaired when light acts upon it. A pigeon, in which M. Longet had removed the hemispheres, and which survived this operation for eighteen days, shewed, when a light was brought in the dark before the eye, immediate contraction of the pupil, often even winking, and the same took place after the removal of the *optic Thalami.*

I have performed on rabbits experiments which gave a similar result. After I had rendered the animal insensible by means of ether, I broke up the skull, raised the brain forwards, after dividing the olfactory nerves, and placed a glass rod below the optic nerves. When the poles were applied to one nerve, the usual contraction of the pupil took place. I then removed by slices the two hemispheres of the brain until the lateral ventricles were exposed. The repeated irritation of the optic nerves had still the same result as formerly; upon each touch with the wire, the pupil became contracted. I then removed both optic *Thalami;* this also did not act on the pupils. I proceeded then as far as to the anterior pair of the quadrigeminous bodies; the removal of their upper layers, which the animal, that in the meantime had recovered sensibility, bore very calmly, had only a temporary action. As soon, however, as the optic Thalami were destroyed through their substance, convulsions and indications of violent pain arose; and no irritation whatever of the optic nerves was capable of causing the Pupils to become contracted. It was exactly the same as if these nerves had been cut through.

Since, then, the experiments of M. Longet have also shewn that the removal of the *Cerebellum* does not destroy the action of light upon the pupil, it may be considered as an established conclusion, that the anterior optic *Thalami* are the organs which accomplish the reflex action between the optic nerves and the oculo-muscular nerves.

The Quadrigeminous Eminences of the right side act upon the left and right Iris; and in like manner there is maintained with the Quadrigeminous Eminences of the left side an action, the real existence of which has been established by the experiments of M. Longet and M. Flourens.

The course of the filaments might consequently be the mode of communication. That is, one part of the filaments of one optic nerve on the same side remains, and through the Optic Tract behind the Optic Thalamus reaches the corresponding Quadrigeminous Eminence, and here its communication with the Oculo-muscular Nerve of the same side is maintained; while another portion of the filaments of the Optic Nerve, by means of intercrossing or decussation, reaches the opposite Quadrigeminous Eminence. It might further be possible, that the Oculo-muscular nerve, which, according to the observations of M. T. Weber and Rudolph Wagner, intercrosses in like manner with that of the opposite side, may be the agent of this transport of action; and lastly, that both nerves may be biparted. Upon this point, however, experiments are still wanting.

b. Besides the reflex action, which may be manifested artificially in the Iris, and which may be observed through the natural influence of the light upon the Optic Nerves, we witness the same phenomena of contraction in the Pupil as an associated action. As often, namely, as the eye moves internally, the pupil, as Johan Muller observes, undergoes contraction. The nervous branch which forms the small root of the Ciliary Ganglion, and is expanded in the circular muscular fibres of the Iris, and which belongs to the *Nervus Oculo-motorius*, receives in this instance at the same time an associated irritation, when the will calls into action other filaments of the same nerve. It is well known to be no uncommon phenomenon, that the stimulus of the will calls into action more filaments than had been intended;

that another part, which it had not been intended to put in action, acts in an associated manner; and that it requires constant practice in order to render possible a perfect isolated action of the nervous filaments. When, however, the associated action proceeds from the filaments which are under the dominion of the will, it may be understood that this takes place still more readily in those filaments which are indifferent towards their peculiar stimulus,—that is to say, which belong to organs which are not susceptible of a voluntary action.

M. Johan Muller has further on this subject observed, that the Pupil also undergoes contraction, when the eye is turned involuntarily inwards, as is the case during sleep, and in looking at a near object.

c. Upon a third mode of Contraction of the Pupil, and its dependence upon the *Nervus Oculo-motorius*, namely, the antagonistic action, the question shall be more fully considered below.

§ II. DILATATION OF THE PUPIL BY MEANS OF THE SYMPATHETIC NERVE.

The first experiments upon the influence of the Great Sympathetic upon the eye, and particularly upon the Pupil, proceeded from M. Petit.[1] These experiments were principally made in order to prove that the Sympathetic Nerve proceeds, not downwards from the Brain, but ascends upwards to the cerebral nerves. Petit accordingly maintains that this Nerve proceeds from the Spinal Marrow, and indeed by as many roots as the rootlets or branchlets of the Vertebral Nerves which are observed to be connected with it. By means of these, the nervous fluid, according to Petit, is received from the members, the abdomen, the chest, and the throat, and is conducted towards the head. This doctrine found opponents almost in all anatomists, who,—as Achillini, Albinus, Haller, and others,—represented the Sympathetic Nerve to arise either alone from the sixth pair of Cerebral Nerves, or from the sixth and fifth pairs, as Willis, Gasser, and their followers.[2]

Upon placing a ligature upon the Sympathetic Nerve in the neck in dogs, Petit observed the *membrana semilunaris* contract itself over the Cornea, the eye occasionally became smaller, flattened, lacrymating, moist, turbid; in one experiment the Pupil became dilated and immoveable as in amaurosis.

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[1] Memoires de l'Academie des Sciences, 1727.

[2] Compare Haller Opera Minora, t. i., part i. 510; Ludwig Scriptores Neurologici, i., p. 257; iii. p. 78, 79. Petit loco citato, p. 2. Haller Disputationes Anatomicæ, tom. ii., p. 871, &c.

In all his other experiments, on the other hand, Petit observed, that after the section of the Sympathetic Nerve, the pupil undergoes contraction, and this is the result which other inquirers also have obtained, who observed carefully the condition of the pupil. Subsequent to the time of Petit, experiments were made upon the section of the Sympathetic Nerve in the neck by Molinelli,[1] Cruikshank,[2] Dupuy,[3] Arnemann,[4] Mayer,[5] Brachet,[6] Reid,[7] Arnold,[8] Valentin,[9] Stilling,[10] Volkmann,[11] Biffi,[12] and other persons.

Molinelli is the first who represents in his observations the Contraction of the Pupil as a subsequent phenomenon; whereas Dupuy describes it as an immediate consequence of the operation.

Dr John Reid has performed upon this subject a great number of accurate experiments, and in dogs and cats he found the Contraction of the Pupil to be a never-failing result, while in rabbits the same result was not so constantly observed to take place. Particularly deserving of attention is the fourth experiment of Dr Reid, in which the sympathetic nerve above in the neck of a cat of moderate strength was subjected to compression. In this instance, the pupil began progressively to be contracted, and became narrower than that of the other eye, and nevertheless recovered its previous diameter as soon as the compression was withdrawn. Dr Reid, in a Note on this experiment, remarks that he had witnessed the same result in other cases. As soon, however, as a portion of the Sympathetic Nerve was cut off, the Pupil became permanently contracted.

According to these experiments, consequently, it must either be admitted, that a stimulus applied to the sympathetic nerve in the cat has the effect of inducing contraction of the pupil, or that the temporary compression of this nerve for the moment paralyses it, and that its power is speedily again restored. The last inference is also the correct one, as appears from the experiments to be mentioned below.

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[1] Commentarii Bononienses, vol. iii., p. 281. 1755.

[2] Philosophical Transactions, 1795. Part First.

[3] Journal de Medecine, tome xxxvii. 1816. P. 340.

[4] Versuche über die Regen. d. Nerven, i., pp. 67–69, 85–96.

[5] Journal von Gräfe und Walther, x., p. 418.

[6] Recherches Experimentales sur les Fonctions du Systeme Nerveux. Edition Deuxieme, p. 432.

[7] Edinburgh Medical and Surgical Journal, Volume Fifty-Second, 1839, pp. 38 and 39; and Physiological, Anatom., and Patholog. Researches, pp. 95, 291.

[8] Bemerkungen uber den Bau des Hirns und Ruchenmarks, p. 122.

[9] De Functionibus Nervorum, p. 109.

[10] Ueber die Spinal Irritation, p. 157.

[11] Wagner's Handworterbuch, iii., p. 281.

[12] Omodei Annali di Medecina, t. xxii., p. 630.

Dr Reid endeavoured further, in a particular manner, to prove that the section of the *Nervus Vagus* has not the effect of inducing contraction of the Pupil. In dogs, cats, horses, and many other animals, the *Nervus Vagus* and the Sympathetic Nerve are enclosed in one and the same sheath ; and it consequently becomes requisite, in order to perform experiments upon the individual nerves, to separate them skilfully, which may easily be done in young animals.

Dr Reid separated with care the Sympathetic Nerve of an adult cat from the *Nervus Vagus* in the left side of the neck, and then divided it. Forthwith the pupil became contracted, and progressively continued to contract, so that it formed a strong contrast with that of the other side, and the third eyelid (*membrana nictitans*) was drawn over the inner surface of the cornea. The right *Nervus Vagus* was then exposed, separated from the Sympathetic Nerve, and its trunk cut through without injuring the latter nerve, and no change was manifested in the pupil of this eye.

Arnold likewise arrived by another way at the same conclusion in regard to the *Nervus Vagus*, in so far as he performed his experiments upon Birds. The Sympathetic Nerve is in birds completely separated from the *Nervus Vagus ;* and, as after section of the latter, no changes in the state of the pupil are perceptible, Arnold with justice assumed that the contraction which Petit and other inquirers beheld in dogs, cats, and other animals, after section of the united *Nervus Vagus* and Sympathetic Nerve, proceeded not from injury of the *Nervus Vagus*, but from that of the Great Sympathetic.

Valentin confirmed the truth of the experiment of Petit, and obtained, further, some new results. This inquirer found, that, after extirpation of the ganglion of the *Nervus Vagus*, the Pupil always is rendered oblong, is no longer round, and very frequently, particularly at the upper edge, that it is angular (*l. c.*, p. 111). He compares the narrowing of the pupil to an act of contraction, and the dilatation to one of extension. " The iris," he remarks, " has a twofold nervous source ; one in the brain, by means of the oculomuscular nerve, and one in the spinal cord." It is further his belief, that the latter source supplies the Iris through the medium of the cervical nerves, which proceed to the superior cervical ganglion and the ganglion of the *Nervus Vagus*. This, however, is not the fact, as shall be shewn below.

" By applying a stimulus to the oculo-muscular nerve," says Valentin further, " the pupil becomes very much narrowed ; —dilated by palsy of the same nerve. By applying the stimulus to the spinal cord filaments, that is, according to the author, to the superior cervical nerves, the Pupil becomes something larger." (E contrario fonte spinali irritato, pupilla aliquantum augetur ; eo vero persecto, fons cerebralis antagonismo caret, ut hac de causa pupilla summopere minuatur.)

M. Stilling also makes the following remark in a Note :— " Upon dividing the Sympathetic Nerve in the middle of the neck, first, great dilatation took place, and immediately thereafter repeated, forcible, and then permanent contraction."

Volkmann, on the contrary, in his experiments, did not observe dilatation of the pupil, in consequence of irritation of the Sympathetic Nerve.

The most recent experiments upon this subject have proceeded from Signor S. Biffi, in whose Essay the following passage relating to this point is found.

"With reference to the Sympathetic Nerve, I began to apply a stimulus to the small filament, which the superior cervical ganglion transmits towards the bifurcation of the carotid artery ; upon this the pupil underwent a little dilatation. When this filament was divided, the usual changes in the eye took place, though only in a slight degree. The dilatation of the pupil manifested itself more forcibly when I applied the stimulus, on the contrary, to the descending cervical twig of this nerve. (La dilatatione della pupilla si mostro maggiore, irritando invece il funicolo cervicale descendente di quel nervo.) When I applied irritation to the superior cervical ganglion itself, the Pupil attained its greatest degree of dilatation, such as took place in none of the instances hitherto mentioned. When I removed entirely this Ganglion, the pupil underwent contraction in less than one minute to the size of a pin-head."

These observations by Signor Biffi are perfectly faithful to truth, and correct, as may be seen from the following representation. (The statement, however, that the Great Sympathetic Nerve above the superior cervical ganglion causes only a little dilatation, in consequence of the application of a stimulus, it is impossible to maintain, because the dilatation from this influence is sufficiently strong.)

These are, so far as is to me known, the most important investigations which have been made previous to those undertaken by ourselves. Individual ones are yet to be mentioned below. From the facts now adduced, it results that it has already been repeatedly seen, that, in consequence of irritation applied to the Sympathetic Nerve in the neck, the Pupil undergoes dilatation, before the period at which our experiments were performed. In short, it must be allowed that Signor Biffi has the merit of having promulgated this as a fact in science. By means of our experiments, not only has this fact received confirmation, but it has also been placed in scientific connection with other facts.

Our experiments were performed on rabbits, dogs, and cats, during life, and shortly after death, and also on the person who suffered death by the guillotine, of whom mention has been made above. For the purpose of experiment, rabbits were the animals mostly employed. All the animals were previously rendered insensible by means of ether or chloroform. The latter agent, however, is dangerous in rabbits and young dogs and cats, as these animals die speedily, and often suddenly. On the other hand, ether presents itself, especially for rabbits, as an excellent means of inducing insensibility, which the operator, with some practice, has, with rare exceptions, completely in his power. The exposition in rabbits of the Sympathetic Nerve, which lies behind the common carotid artery, at the inner side of the *Nervus Vagus*, close to the larynx, is so easily effected, that I have not thought it necessary to describe the process minutely. The isolated nerve is then placed upon a glass rod, and touched by the two poles of a galvanic apparatus; the Pupil begins immediately to undergo gradual dilatation, until it has attained its most extreme degree of expansion. To behold this change is very interesting, as the margins of the Iris separate from each other more and more, and a progressively larger surface of the lens becomes visible. We had at first employed, for the purpose of applying irritation, the rotation apparatus; but subsequently, after Professor Mayer had provided for the Anatomical Institution of this place the galvano-magnetic apparatus so appropriately improved by M. Dubois-Reymond, and had placed it at my disposal, we employed this excellent contrivance. The amount of force which was necessarily employed in rabbits, in order to excite the action, is extraordinarily different in the several individual animals, and it is not in our power to state upon this point anything definite. We have many times found that Dilatation had already taken place, when we could scarcely or not at all perceive the presence of the electrical current by the tongue. Often, however, I must have employed a greatly stronger force in the experiments which I performed.

It is possible, on the other hand, with a feebler degree of force, to produce the same result when the irritation is continued longer. The above-mentioned apparatus of Dubois furnishes the great advantage of making a progressive diminution of the current; and when the instrument is provided with a scale, it is possible, by trials in which we arrive at a comparative determination of the force of the current, to make many averages, without employing any particular apparatus for ascertaining the force of the current. I have marked the spot at which the action is strongest, with the cypher 1; and I have selected a division into centimetres and millimetres.

In order to illustrate the above-mentioned result, I give one single experiment. In a rabbit I beheld already in half a second, the dilatation of the pupil take place, when the index pile stood at nine centimetres; on the other hand, it continued for two seconds at fifteen centimetres; and the result remained at twenty-three, even after the lapse of ten seconds.

Some time back I performed experiments upon the crural nerves of the frog, and then made the observation, that there is in one and the same nerve a point which is more or less irritable to the same irritative action, although the nerve has been treated in all parts in the same manner. It not unfrequently happens, that, when the operator applies a stimulus with the same force to two points, one of which is distant from the other only two millimetres, from the irritation of the one no trace of convulsive movement arises, while from the irritation of the other, decided reaction takes place.[1] Upon the same subject, I have also performed on the Sympathetic Nerve in the neck, experiments which I shall here state.

There were six experiments, all made upon rabbits. The Sympathetic Nerve was exposed on one side along the whole neck to the superior cervical ganglion, so that it lay there perfectly unconnected. The loss of blood during the operation was almost none. Branches of the carotid artery which interrupted the operation, were enclosed in ligatures, and this was uniformly done so far as was seen to be possible, that the nerve itself during the act of exposition might not be touched. The animal had been previously subjected to the full operation of ether. The nerve was then measured, as far as either the lower edge of the upper cervical Ganglion, or the lower edge of the thyroid cartilage serves as a boundary. Beneath different spots of the nerve a glass rod was placed; and sometimes below, sometimes in the neighbourhood of the Ganglion, the application of irritation was commenced. The strength of the current was, by pushing back the posterior pillar of the apparatus of Dubois, so much diminished, that the operator no longer felt the current with the tongue; irritation was then applied. In the mode of applying the irritation the procedure was always in the same way in the same experiment; that is, either so that the portion of nerve lay upon the wire, or that one wire touched the lower surface, and the other wire touched the upper surface of the nerve. It is good to observe this condition, because, naturally, a great deal depends upon the greater or less completeness of the contact. So also it is, for instance, not a matter of indifference, when the operator places the nerve upon the wire, whether the same covers the tips of the wire, or is placed further removed from them.

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[1] Frorieps Tagsberichte. Anat. und Physiologie, 1852, April, p. 348.

The nerve must be kept constantly wet, and hence it requires to be moistened from time to time.

The duration of the irritative action ought naturally not to be different, because a spot less irritable nevertheless gives as good a result as one more irritable, if the first be subjected to irritation for a longer time.

It is further requisite to rest from time to time, in order to allow the nerve again to recover itself; and lastly, it is requisite to irritate no one part oftener than another, in order to keep the degree of irritation at the same amount so far as is possible.

In the application of these general rules, I observed in the six experiments which I performed upon rabbits, that the portion of the Sympathetic Nerve which lies near the chest, is in general less irritable than that which is situate higher up. Thus, for example, in one experiment the two piles were removed from each other twenty-four centimetres, and then irritation was applied to one spot of the Sympathetic Nerve, which lay thirty-two millimetres below the lower edge of the thyroid cartilage; the pupil underwent not the slightest alteration, even after the irritation was long continued. After a short pause we applied the wire to a point eleven millimetres higher up, that is to say, twenty-one millimetres distant from the lower edge of the thyroid cartilage in the nerve, and forthwith the pupil was very considerably dilated.

The pile was then pushed about one centimetre farther back, in order to diminish the force of the instrument, and to find the degree of force at which the portion of nerve mentioned no longer reacted. At twenty-five centimetres, in truth, already all action ceased. On the other hand we could produce the dilatation of the pupil, indeed, by the same force, as a spot still higher was selected, which was distant only ten millimetres from the lower edge of the Thyroid cartilage. When I made a still nearer approach to the superior cervical Ganglion, I found directly below this Ganglion a spot more void of irritability, while the Ganglion itself, again, reacted more forcibly.

In the other experiments I always found that it required less force when the irritation was applied above, than when it was applied below. The ganglion evinced its presence not only by great irritability, but also by this circumstance, that it retained the same for a longer time. Thus it took place once, that after it had been already subjected to irritation for a long time, the cord of the Sympathetic Nerve below the first ganglion could no longer be brought into reaction by applying the greatest degree of irritation, while the slightest touch of the ganglion itself produced effects. The continuation of the Sympathetic Nerve above the first cervical ganglion, I also subjected to the same sort of proof. I have not, however, been able to find that to its excitation a greater degree of force is requisite than to that of the ganglion itself, as appears to result from the observation of Signor Biffi.

From the observations now adduced, it follows, that in the cervical portion of the Sympathetic Nerve, the same phenomenon essentially takes place, which I have beheld in the crural nerves of the frog, namely, that in one and the same nerve a different condition is induced from the same irritation. It appears, indeed, as if the Sympathetic Nerve requires for its excitation so much more force, the nearer it lies to the region of the chest, or the more distant it is from the eye. So much follows from the observations adduced. But I will not place too great weight upon this expression, and I hazard no explanation which may be deduced from the greater or shorter distance of the nerve from its beginning or its termination. I have, in short, not yet examined every small spot upon this rather long tract, and must leave this point for farther researches. However simple this matter appears in representation, it is still opposed with many difficulties, some of which only I here name. Suppose the operator has completely isolated the whole nerve from the first cervical ganglion, and this part at the same time, to deep down in the neck, he is still not able to maintain a like degree of tension in the nerve, and thereby not able to obtain an accurate measurement. The movement of the animal, the movement of the hand of the assistant who holds the head, readily alter the degree of tension of the nerve. But it is chiefly the degree of Irritability of the nerve itself, which leads to variable results. It is astonishing how quickly the Irritability is altered, how speedily, often, it is extinguished. While we think how much depends on this, it is easy to conceive, that it is only after a very great number of experiments has been performed, that it is permitted to determine whether the sensibility of the nerve constantly increases from below upwards to the vicinity of the superior ganglion, or whether also strongly irritable spots are found deep below.

From my own investigations on the crural nerves of the frog, it may be inferred that, in general, the Sensibility is greater in the neighbourhood of the Spinal Cord, and diminishes towards the periphery; while, in the Sympathetic Nerve, the irritability appears to increase towards the periphery, so far as observations hitherto have taught. Only one spot could I find which makes an exception to this, namely, immediately under the ganglion. This spot, accordingly, might be one of those marked by me with the name of Knot-Spots. It is to me probable, that, upon further investigation, the number of these knot-spots may be found to be increased. But as this also may be an exception, the general phenomenon that different spots of the same nerve may be differently sensible to the same stimuli, is a fact of great interest; and the Practice of Medicine is sure to find sufficient materials which may be associated with this. We observe not unfrequently, in cases of Neuralgia which follow the course of one Nerve, the same phenomenon,—namely, spots with excessive pain close beside others which are little affected. I will not say that we can explain these phenomena by the fact now mentioned; but in forming a judgment upon this, we may have to direct an eye to this point, so far as is necessary, that, in the Practice of Medicine, serviceable facts may be collected.

One observation by Signor Biffi may find its explanation by means of my experiments. In the Essay above referred to, this author states, that when he had applied strong compression in a dog to the two lower thirds of the Sympathetic Nerve, with the *Nervus Vagus*, no particular action took place in the Pupil of the eye on the side on which this sort of irritation was applied; the Pupil had been dilated and contracted, and shewed an oscillating movement. But even the same might also have been observed in the pupil of the opposite eye. This phenomenon he explains as a consequence of the pain which is occasioned by the irritation of the nerve. When, on the other hand, he applied irritation to the upper third of the same nervous cord, the oscillating movement of both eyes was indeed manifested; but the pupil on the side on which the operation was performed, became much more dilated, and remained double the size of the other.

It is easily understood, that, in many instances, a mechanical irritation applied to the lower part of the Sympathetic Nerve, remains without consequence, as its Irritability at this spot is smaller in degree.

(*To be concluded in next Number.*)

ART. II.—*Observations on the Dissimilarity and Non-Identity of Syphilis and Gonorrhœa; with a few characteristic marks of discrimination.* By JOHN JOHNSTON KELSO, M.D., &c., Lisburn, Belfast.

There are two hypotheses which have continued to divide the profession, respecting the relation which Syphilis should hold to Gonorrhœa. According to the one, the poison of gonorrhœa and the venereal virus are the same; or both diseases result from, or are varieties of, a given morbid poison. According to the other, both diseases are dissimilar, and originate from different and distinct poisons. On each side of this intricate and difficult question are ranged the names of eminent men; but had they declined more from controversy, and observed better the phenomena of the diseases, this might long since have been settled. But unfortunately the question does not occupy a much different position from that which it held in the time of Mr Hunter. Not that able men were idle in the discussion, but, prejudiced in favour of current doctrines, shrunk from proper pathological inquiry. The controversial spirit of the past times brought into the argument more empty observations and party reprehension, than sound pathological facts. This, then, tended to retard the settlement of the question. And in the outset I would observe, that, however desirable it might be to have proper definitions of these diseases, and the relations which they respectively hold to each other, it would be still more so to have specific and determined rules to regulate their treatment. But as the attainment of the latter object is essentially founded on the former, this will presently come to be considered.

Of the origin of these diseases it must be confessed we know but little. Nor, practically speaking, does the subject deserve investigation. But syphilis, from authoritative testimony we may infer, had a priority of existence. Half a century had almost elapsed before gonorrhœa was noticed, or at most set down as a supposed symptom of the former disease. The merit of the discovery, in the popular opinion, is due to Fallopius. Nosology has not made that progress as to allow correct definitions of diseases; but it is a field worthy of cultivation. Much, no doubt, is to be done first by physiologists.

About the conclusion of the fifteenth century, history informs us of the existence of the venereal disease. Whether its existence in Europe antecedent to this date, or its importation into the continent in 1494–5, be admitted, one thing is certain, the appearance of a new disease,—new in so far as medical testimony has recorded,—in the beginning of the sixteenth century. This time may be considered an era of some moment in practical medicine. Ever since, it has continued to infest those who have addicted themselves to the indiscriminate indulgence of the sexual passion. It is extended now to almost every country, and its ravages are confined to no particular class of persons, nor grade of society. It is, then, a general disease.

How far the immoral practices of men, *en masse*, have contributed to the propagation of syphilis during these last three centuries, I will not stop to inquire. Suffice it to say, the advancement of literature, the enthusiasm of religionists, and the more general dissemination of instructions to the inferior classes, have all in combination contributed but little to establish a better system of morality, and hence to set limits to the diffusion of the disease. It has continued to spread, and it is not difficult to perceive that, unless the nature of men be revolutionised, it will yet farther continue to do so. Of how much practical interest is it, then, to inquire into its nature, its laws, and special mode of action. And although we have a specific therapeutic in mercury, yet were we interested on no other account, our investigation would be more than repaid were we to discover a practical rule to guide us in the administration of this mineral; or, in the knowledge of sure marks, by which we may distinguish between those cases which are in so far allied to true syphilis as to require a mercurial practice, and those which are so decidedly different as to demand quite an opposite and distinct method of treatment. Practical men of our day need scarcely be told of the evils which have accrued from the too often unnecessary and injudicious use of mercury. But there are yet some routinists,—the unfortunate result of cases too often testify,—who use this medicine with a too liberal hand. Yet, on the whole, this speculative practice of the Hunterian school,—for I can call it nothing else,—is fast on the decline.

Half a century had nearly elapsed, after the memorable appearance of the venereal disease, when there was manifested another disease, attacking the same organ, though confining itself to a different texture; attacking male and female equally, though exciting a different action; and being amenable to a different and quite an opposite method of treatment. This affection has received the etymologically erroneous appellation of *Gonorrhœa*. It is a distinct substantive disease, propagated by unhealthy intercourse of the sexes; holding certain obvious relations to the system; influencing, and in turn influenced by particular states of the same system. It is also contagious, but not communicable from the parent to the fœtus *in utero*, or child. This is one great fact. Its morbid action is confined in general to the mucous structure of the urethra, but occasionally extending to the bladder and testicles, evincing phenomena decidedly inflammatory. The gonorrhœal poison acts on no other structure but mucous, and on that of the urethra much more easily than on others of a like nature, as for instance the intestines, &c. This is the next fact.

However different both diseases may be in their actions, &c., they both agree in being contagious; but in this respect they are not singular. Both are regulated in their general actions by certain peculiar and distinct laws; and while they occasionally alter or modify the healthy or natural actions of the system, are in turn altered or modified by special, often indistinct, states of the same system. The presence of both in the same individual, are of frequent occurrence, and, as shall afterwards be proved, originate from a plurality of poisons. The one disease has not, nor can have, any connection with the other. Nor can the one even excite the other. They are influenced by opposite laws, and tend to opposite results. The one acts on the constitution by continuance as a contagion, which deranges a multitude of its actions; the other locally, in the same light as a phlegmon, but more or less in proportion to the inflammatory action. The one, in short, comes to be a disease of the system; the other is merely local, performing the action of the new and unnatural texture. Both require opposite and distinct remedies in their treatment. And I may remark, that I am only *echoing* the sentiments of the majority in the profession, when I say that the practice which benefits the one, rarely, if ever, acts beneficially on the other, but occasionally aggravates it. This, then, I take hold of as an important characteristic discrimination between syphilis and gonorrhœa. It is a fact not yet negatived by the assumptions of any man, or class of men. And the majority in the profession have arrived at the unanimous opinion, that the recent champion, in Mr Wallace of Dublin, in favour of an identity of treatment in both diseases, has failed to establish his point, or bring over a single proselyte to so untenable a doctrine. The ingenuity of a Hunter, even, shrunk from establishing a rational explanation of this discrepancy of practice. Nor have his followers been more successful, although, perhaps, a little more speculative.

In examining this question I am led to a more particular consideration of what Syphilis is. And I confess I have little to add to what is generally known, that a peculiar disease results on the application of a peculiar morbid poison to the cutaneous and muco-cutaneous envelope of the penis, &c., requiring,—if not a specific,—a particular mode of treatment. This is saying much in little, and must be obviously very vague. But the fact is, I will be happy to hear of something more explicit. We shall presently describe (and, perhaps, with some precision) the immediate effects of altered function; but we are still to look into the peculiarity of the poison, and its special mode of action. It may here be put as a preliminary question, Does action or organisation become first morbidly involved? If we are to admit the doctrine, which it will not be easy to controvert, that action and organisation accompany and modify each other, we must say that altered action will presently lead to alteration of texture; that the new and unnatural texture will keep up the action for some time which called it into existence; that, in fact, altered texture succeeds to disordered action, or rather the latter takes the precedence.

Leaving these points open for future illustration, we observe that there results a peculiar action, on the application of the venereal poison, which is characteristic of primary syphilis. Some time afterwards a sore is produced, which has been incorrectly denominated an ulcer. This performs the suppurative, not the ulcerative action. Granulations, healthy or the reverse, make no part of it. The presence of these are essential to constitute an ulcer. But we are to attend a little more particularly to the actual changes which precede this. Observation and experiments have proved that the continued application of the poison to a particular part is indispensable to the production of the disease; but the precise time necessary has not yet been accurately ascertained. On an average I would say twelve hours; but there is considerable latitude. The quantity and activity of the poison, to say nothing of the constitution, must make the exact time always vary.

Next, the immediate effect of the poison is not actual inflammation, but a state merely approaching this, which has been called congestion. Were the poison more concentrated, and in greater quantity and longer applied, the action might go beyond mere congestion of the capillary vessels. There might, in fact, be considerable excitation of action; but this is a state quite different from inflammation. Inflammation is a new and unnatural action, leading to and connected with a new and unnatural organisation. It consists in the complete subversion of the actions of the part, whether preservative or functional, with the natural organisation. It is, in the strictest sense of the term, a diseased action. If this view of inflammation, which is not quite novel, be correct, no one can presume to maintain, that this process is in any way connected with the production of a primary syphilitic sore. In irritable constitutions there may be, and is, occasionally a greater degree of excitation, but still there is no inflammation. Congestion, or at most excitation of vascular action, is, then, the immediate effect of the chancrous virus.

But this state does not long continue. Action and organisation go together, we are told, and modify each other. Presently there results effusion, either of a serous or albuminous fluid; occasionally it is fibrinous, mixed with blood. This goes on to increase, and immediately we have the formation of a pimple or vesicle. But this, from the thinness of the covering and the continued increase of the contents, is presently ruptured. This is often done wilfully, from some degree of itchiness which attends it, or by accident. Occasionally the effusion desiccates into a scab when it is detached. But in whichever way the covering is detached, there is one and the same result,—a suppurating sore. This performs a peculiar action, and exhibits particular characters, which, in general, characterise it from other sores. This peculiar action exists independently of the suppurative one.

Every sore in its commencement,—I am speaking of those which have been called, for the want of a better name, specific,—consists of two actions,—one a peculiar action which called it into being, and the other a general one, which has been called by Professor Burns the suppurative, not the ulcerative, as it wont to be. The probable justice of this distinction I conceive is supported by the phenomena of the new texture or growth. Were there no theoretical distinctions like this,—for I cannot positively prove them by analytical or synthetical demonstrations, but the existence of which are obvious,—every sore would be alike curable, or convertible into the ulcerative action, from the cancerous down to the simple phlegmoneus. But the history of many such cases unfortunately rebuts this conciliatory doctrine. All these sores have a peculiar,—I will not call it specific,—inherent action, notwithstanding their palpable one, by which they are especially characterised. This might in contradistinction be called their *latent* action, as we can only judge of its presence from its effects. A syphilitic sore, in its action and aspect, is peculiarlydifferent from a cancerous, and this, again, from a sloughing one; but all agree in furnishing a not very dissimilar matter. And if we are to adopt the more correct view of suppuration and ulceration, as laid down by Dr Burns, we must say that the primary palpable action of every syphilitic sore is the suppurative. Ulceration is quite a different and subsequent process. But suppuration is a diseased secretion; so long as it continues no sore can heal. Every sore must ulcerate previous to healing. It should then become a practical duty to obviate the first of these actions, and encourage the second. And this is precisely the practice of any rational man, although he may not be just aware of the action of his escharotic applications.

We have now seen that the primary palpable action of every sore is the suppurative; an action by which a peculiar fluid is formed, which has been called its secretion. But this continues to be furnished in greater or less quantity, whether the sore be suppurating or ulcerating, but in different stages exhibiting various qualities. From the appearance of the secretion, men have been in the habit, from remote times, of judging of the character of every sore. An alteration in the quality of the matter is a good criterion of the progress which the sore may have made towards healing, and *vice versa.* But I cannot prosecute this subject farther than briefly to inquire a little more particularly into the action by which the secretion is furnished. Secretion has been defined to be the result of a special action of the extreme arteries, or capillary system of vessels. But how far it may be attributed to this source alone appears questionable. I would be rather inclined to take up the negative ground, and say that the fluid thrown out by the arterial vessels is modified by absorption. The absorbent vessels, I need not remind my reader, are found to be present in all parts of the body, and in every sore. They are not, I confess, easily demonstrated, but we frequently see their effects, occasionally to a fearful extent, as in the sloughing sore. That so extensive a system of vessels should remain idle, in any case, is not in accordance with their function. They may be, and are, occasionally excited to augmented action, but to say that they should remain quiescent in any case, especially in a new and unnatural texture, would be just as much as to say that the part had lost its vital properties. So long as a part is connected with the general system, and is influenced by its laws, whether the organisation be or be not altered, a particular part must be taken up in place of another particular part which is deposited. This constitutes the vital actions of the part. Every sore, then, is possessed of vitality, sometimes to a greater, sometimes to a less extent.

But it would be equally erroneous to say, that the absorbents which are spread over the surface of every sore were only destined to take up the particular fluid, the presence of which would have obviated the formation of the peculiar secretion. Part of them only performs this action, while the remaining part perform their usual function. Those destined for the modification of the secretion, it is presumable, modify it according to their activity. That the capillary vessels may and do modify a little their contents during its passage through them, I am not prepared to deny; but to attribute the formation of the peculiar secretion to their sole action would be unphilosophical and assumed. And until it be positively proved that the absorbents make up and perform no important function in the formation of every secretion, I am justified in giving the negative to the assumption,—for I can call it nothing more,—that the capillaries solely perform this action. I attribute to the arteries their due, when I say they modify a little their contents circulating through them. The absorbents do the rest; but I cannot positively state the amount of action which they perform, or the particular fluid which they at particular times take up. The whole process is, no doubt, under the influence of nervous energy; but to attribute all, again, to this source, would be to give up the investigation. And as there was formerly too little attributed to nervous influence, there is now, perhaps, too much.

As soon as any sore is produced, we observe the commencement of the process of secretion. And if the view which has been taken of this be correct, I would say, that whatever was the character of the sore, the absorbents in modifying the secretion would also take up some of the particles of the matter which brought the sore into, and kept it in, existence. And in point of fact, we do find this to be the case, and hence the necessity in syphilis, and other sores equally specific, of subduing morbid action early.

I have now only to observe, that a syphilitic sore may be produced, on the application of the infection, in any cuticular or muco-cuticular structure. By a continuance of the local action, the general system comes to be involved, producing what has been termed lues venerea. Particular structures become thus involved in a secondary way, effecting, if not checked, hideous destruction. But I am not to inquire into the cause of this, nor the channel through which it is effected. This disease is communicable from parent to the fœtus *in utero*, and to the infant through the medium of the suck. This is an important and well-ascertained fact.

Next, Gonorrhœa presents itself for consideration. This is a peculiardisease,—peculiarly dissimilar from syphilis. Its action, I have to repeat, is principally confined to the mucous structure of the urethra. And if we go a little farther, so as to inquire into what this action is, it will be found to consist essentially in inflammation. We have seen in what the syphilitic action chiefly consisted; that inflammation entered into and made up no part of it. The first stage, it has been submitted, was that rather of congestion, but this is a state quite different from inflammation. Nor in the progress of a syphilitic sore have we a continuance of this congestion. In irritable habits from untoward circumstances, we have seen, there might be vascular excitation, but this is one thing and inflammation another. It is only a precursor of this state. On the other hand, the gonorrhœal action is manifestly inflammatory,—inflammatory, more or less, from first to last. This state is evinced by an increase, as well as alteration, of secretion. But there are other concomitant symptoms characteristic of inflammation;—pain, heat, œdema, and redness. Pain is a chief and generally primary symptom of every gonorrhœa. There is invariably a feeling of increased temperature, and œdema is occasionally evinced by the contracted, often spiral form of the stream in micturition. And could the urethra be inspected in this stage, we should be able, no doubt, to demonstrate redness. We judge of the continuance of inflammation from the discharge and its qualities. As soon as any mitigation of the former takes place, we have a marked diminution of the latter. Nor is this ever excited without a manifest cause. Thus, inflammation is the immediate effect of the gonorrhœal poison. But what the poison consists in, and in what it differs from the syphilitic and vaccine poisons, I cannot tell. It is sufficient we know its action, and the means by which this may be counteracted.

Such is a brief view of some of the discriminative characters of Syphilis and Gonorrhœa, and their special modes of action. If we are to have a nosological definition of a disease from its symptoms or cognisable phenomena, we must say that syphilis is one disease, gonorrhœa another; the one effecting disorganisation of texture (I speak of their primary local effects), the other augmenting action, producing an increase and alteration of secretion; the one manifested and attended by no inflammation,—at most congestion;—the other by decided local inflammatory action; the one acting so on the system as to produce what has been called Lues Venerea or secondary symptoms; the other possessing no such characteristic properties. They are attended by different symptoms, regulated by different laws, and tend to opposite results; and were we to institute an experimental inquiry respecting their treatment, we would find that, by keeping up in both cases the action of the bowels, in constitutions not dissimilar, syphilis, on the one hand, would, in nine cases out of ten, be an obstinate, in fact, an incurable disease; while, on the other, gonorrhœa would be in general mild, and comparatively harmless. In point of fact, the one is a disease which soon becomes connected or identified with the system, influencing many, and quite altering some of its functions. On the contrary, the other is merely linked, if the expression be admitted, to the system, not just in the same way as a parasite or new growth, but in so far as the local action is more or less inflammatory. To infer that they are the same disease, or originated from the same poison, would be just as philosophical as to say an eruption on the chest was in any way connected or identified with a pleurisy, because they occurred at the same time. These are connected in so far as they occurred in the same system, but this is quite different. So it is with syphilis and gonorrhœa. Because they attack the same organ, we are not on this account to infer that the one disease is a modification of the other, or that a difference in organisation would so modify a given morbid poison, as to make it assume in one case a syphylitic action, in another a gonorrhœal one. What philosophy could illustrate so monstrous an anomaly? All diseases might originate from a few morbid poisons! A doctrine so speculative, so superficial, and especially when so opposed to experience, cannot for one moment stand the test of scrutiny.

My space will not allow a complete examination of the doctrine of those who support an identity of diseases, or unity of virus. On this side of the question are ranged many able men; but so diversified phenomena as are exhibited in the two diseases could not be reconciled, even by Mr Hunter. He advocated with much ability the question, that difference of texture effected a modification of the venereal poison; but pathological facts are opposed to his inconclusive arguments. His experiments have subsequently been repeated, but with a different result. His best admirers have even confessed them to be erroneous. Sawrey, Swediaur, Adams, &c. &c., have followed on the same side. They have bent many facts to accordance with their opinions. But pathology must be a delusion, or syphilis must be a separate and distinct disease from gonorrhœa. In labouring to establish my view of the question, I am not so much actuated with a desire to erect nosological distinctions between these, as to the rectification of a too frequent, I fear, mistaken treatment. If gonorrhœa be to be treated by mercury, as Mr Wallace inculcates, let the question be determined. But practical experience must precede speculation, and this goes decidedly to negative so dangerous a doctrine. But I wish not to be misunderstood.

In this sweeping condemnation of mercury as a therapeutic for gonorrhœa, I do not include a mercurial purge. If it be to do good in any case, it is through its action on the bowels; in proportion as it moderately purges, it effects a beneficial result.

"But mercury," says Swediaur, "cannot cure gonorrhœa, because it is out of the course of the circulation." Is the urethra out of the course of the circulation? Happy system of physiology! A chancre on the extremity of the glans penis must be still farther out of the circulation! Is the exhibition of mercury, then, in syphilis of no utility? Is gonorrhœa amenable to no practice?

Another question of importance arises. Can syphilis be transferred to a mucous texture—the urethra, and *vice versa*, gonorrhœa to a cutaneous one? An extensive experimental inquiry could only satisfactorily decide this; but so far as a very limited number of experiments of my own go, and from what are on record, I am entitled to reply in the negative. Hunter believed he excited a chancre within the urethra; so did Benjamin Bell; but I disbelieve in the possibility of the occurrence, so far, at least, as actual contagion operates. What might be excited by experiment I cannot tell, but concur in the practical observation of Mr Samuel Cooper, "that I have never seen a chancre within the urethra."[1] As to the matter of gonorrhœa exciting this disease in a cuticular structure, I have briefly to say, that experiments have hitherto failed to produce such a phenomenon. I have mentioned experiments as the most probable way of any, for effecting this; for as to the possibility of it occurring from mere contagion, I have long discarded the very idea as irrational. And practical experience gives confirmation to these experiments, from what occasionally occurs in cases of these diseases. In gonorrhœa the prepuce not unfrequently becomes swollen from an effusion of bloody fibrinous fluid, to such an extent as even to cover the extremity of the urethra. By this means the matter becomes entangled, and freely introduced under this cuticular duplicature; yet the testimony of able men, and practical observation, has hitherto gone to negative the supervention of any affection bearing the remotest resemblance to gonorrhœa. This is a well-ascertained fact. Syphilis, again, is often attended by the same œdematous state of the prepuce, frequently to such a degree as virtually to close the orifice of the urethra. By this means the free secretion of a chancrous sore comes to have free access to the extremity of this canal, but no one has borne unequivocal testimony to the supervention of a syphilitic sore within the urethra. This is the next fact. But it will be immediately inquired, Does not the one disease frequently occur during the existence of the other? Whence is the cause? I reply to the first of these questions in the affirmative; and with respect to the second, have to observe, that the contaminating party was labouring under both diseases. This point, it may be responded to, is one assumed; we have no proof-positive of its authenticity. I can only answer, as there was no actual examination of the vagina, that the assumed position cannot be disproved. And if the co-existence of the two diseases, as I will afterwards endeavour to establish, in the same individual be not incompatible, he must be a sceptic, and fond of speculation, who would revert to the doctrines of Mr Hunter or any man for a more rational explanation. I will not occupy the pages of a valuable Journal with a refutation of the explanatory jargon of Sawrey, respecting the reason why chancre does not generally, in the same person, produce gonorrhœa, and gonorrhœa chancre. When he appeals to chemical attraction, as drawing the poison from the mucus to the urethra, and from pus to the dry parts, he appears as clinging by the last timber of so imbecile a wreck. Nor are the tottering arguments of Swediaur more in point. "The contaminating poison of gonorrhœa cannot be absorbed into the system," says he, "because it is out of the course of the circulation"! Is the urethra an isolated structure,—isolated from the general system, in so far as not to be supplied with blood? Is this imbecile doctrine in accordance with its function? Next, we have Adams telling us, in a learned disquisition, of the existence of gonorrhœa in the time of Moses; and concluding from this, that so far from considering them as different, the fairest inference may be drawn, forsooth, in favour of their identity!

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[1] This was seen by the late Herbert Mayo. See Edinburgh Medical and Surgical Journal, Volume Seventy-Eighth, p. 425, 426.

To Benjamin Bell senior is due the merit of being the first who questioned the doctrine of Hunter. He instituted experiments; and from a difference of result as obtained by his predecessors, inferred that the poison of syphilis and gonorrhœa were dissimilar. This question has continued to agitate physicians since his time. Many able men have ranked under and contributed their powerful support to this side of the question; but it appears yet, in the eyes of some, to want farther elucidation. Bell, it has been observed, erred; or, what is equally to the purpose, subsequent experiments have not confirmed his. The syphilitic poison being introduced, the urethra, we are told, produced a chancre; but inflammation, I conceive, must have been mistaken for a chancrous sore. Be this, however, as it may, the experiment proves one point;—that the poison of a primary syphilitic sore is incapable of exciting a gonorrhœal discharge. And the result of Hunter's experiment, if it can be relied on, proves the same thing. The next experiment was made with the matter of gonorrhœa, to ascertain whether it was transferable to a non-secreting surface. Nothing resulted on the insinuation of the poison between the prepuce and glans penis, but a slight inflammatory action, which soon subsided. This decidedly proves the next point; and subsequent experiments of my own have confirmed this. If to these we add those of the students, which cannot but be admitted, we are warranted in arriving at the obvious conclusion, that there is a dissimilarity of diseases, or non-identity of virus. In arguing this question, I am guided chiefly by facts and experiments; and this, I would observe, should be the basis of every disquisition of this kind.

But the watchful critic may exclaim, " Are there no other questions yet to be determined ?" I have only to reply, there are none within the pale of a pathological explanation. It has first been argued, that the co-existence of the two diseases are incompatible. This opinion was founded, no doubt, on the supposition of Mr Hunter, that the presence of one disease rendered the adjacent parts less susceptible of the influence of another. But nothing is more fallacious, and especially so, as far as relates to local diseases. As well might we say, that a chancrous action in a part of the system would be a preventive against a syphilitic action in another part. The action of the one has no connection with that of the other, for both diseases, by the assent of many able men, are in their commencement local; at any rate, the latter is especially so. So it is, *mutatis mutandis,* with syphilis and gonorrhœa. And if it be proved that syphilis be a distinct disease from gonorrhœa, and is incapable of producing it, we must say that a person labouring under both diseases, —for there are many such cases,—has received both poisons. The contaminating party, then, must have been labouring under a plurality of diseases. Thus, one of two things comes to be admitted,—that the syphilitic virus is capable of producing gonorrhœa, and, *vice versa,* the gonorrhœal poison syphilis; or, that a communication of both diseases can be effected at the same time. The former position I cannot but consider as unequivocally disproved : the validity of the latter must then be admitted. Every day cases of a co-existence of the two diseases present themselves; and it may not be out of place to reiterate, that the mercurial practice necessary for the cure of a syphilitic affection rarely benefits a gonorrhœa when this co-exists.

Next, we observe a variation as to external characters in primary syphilis; but these are all referable to a common generic head. Because, in the present imperfect state of the science, we cannot form a correct and unobjectionable classification, which would include every species, and those only, we are not to take subterfuge under the superficial plea, that the natural actions of the part are in fault. We should confess our ignorance of the varieties of primary syphilis, and the causes which affect these: and possibly, when pathology shall have made such progress as to allow an explanation why inflammation of a particular part or parts of the intestinal mucous tube will produce a particular eruption,—and a difference, perhaps, in the intensity of this will give a modification of character to the latter, with other multiform intricacies in medicine,—we may be able to furnish this desideratum in practical medicine. But variations as to character in primary syphilis, however many or complicated, can have nothing to do with the question under consideration.

We come next to dispose of the cases recorded by Mr Evans, of elevated ulcers occurring from a gonorrhœal contamination. And first, it may be inquired, Was it correctly ascertained in these cases that the contaminating party had merely gonorrhœa? If this be the fact, I have simply to say, that these elevated sores were not in any respect allied to genuine syphilis. But a doubt strikes across my mind, that they were not the mere product of simple gonorrhœa, but referable to altered secretion, perhaps breach of texture, of some unobserved part of the vagina. Mr Evans has admitted the former, nor can he positively deny the latter. An examination of the entire vagina could only have placed the question beyond the reach of scrutiny. But this, be it recollected, was not done, nor will Englishwomen always submit to it, although it is now practised to a considerable extent by some of the Parisian physicians. The tenor of Mr Evans' remarks lead to a farther admission, that the "ulcus elevatum" occurred sometimes spontaneously. This is quite true, and decidedly proves it to be an affection quite dissimilar to true syphilis. I have not unfrequently observed an eruption like this "ulcus elevatum,"—for I do not positively know, far less can define, what this gentleman has restricted the term to—occurring spontaneously in the glans penis, &c., in persons whose morality was unimpeachable, simulating primary syphilis—at least that species of it which comes oftenest under my observation—so far as for some time to be scarcely discriminated from it, but yielding in a day or two to mild, cooling lotions. These sores I regard as the effect of altered secretion, and having no alliance whatever to syphilis: and the important fact is admitted, that they do not taint the system, but if so, as the consequence of the exhibition of mercury.

With respect to Mr Guthrie's belief, that ulcers occasionally occur on the penis from the matter of gonorrhœa, and gonorrhœa is caused by the matter of the same ulcers, I have only to say, these ulcers, or rather sores, are not syphilitic. They have no resemblance, in character or action, to true primary syphilis. And I may now observe, that my principal aim in drawing, as far as practicable, a correct line of demarcation between syphilis and gonorrhœa, and laying down pathological definitions of these diseases, has been to advance to that perfection of being able to discriminate what are the varieties of syphilis and gonorrhœa respectively, and what are their differential symptoms or phenomena; and also, if these parts are not liable to other eruptions, which in their general characters may simulate these diseases. But a minute illustration of this subject cannot be attempted in the present paper. This affection alluded to by Mr Guthrie is, as the "ulcus elevatum" of Mr Evans, the effect of altered secretion; and if they are excited by gonorrhœa, and are capable of reproducing it, I am only the farther borne out in this opinion.

I might go on, did my limits permit, in controverting other points equally superficial, but will content myself with what has been already said. And with respect to these varieties, I would ask, Will not the human constitution modify local diseases? Will the same quantity of infection, in the same constitution, at different times, always produce an equal quantity of disease? Do the exanthemata exhibit always a similarity of character? Assuredly not. And if primary syphilis do exhibit varieties of character, its secondary effect on the system is indicated even by a greater multiplicity of variations. But that the varieties of primary syphilis should excite corresponding ones in the secondary symptoms, is by no means probable, though they may occasionally do so. And as we cannot tell, *a priori*, the peculiar modifications which the former may assume from the organisation of the parts which it attacks, so in constitutional eruptions we cannot approximate to this degree of perfection, nor say correctly which part will become most involved. But these observations are quite out of place, as they do not bear on the subject before us. And, in conclusion, I submit to the decision of the impartial part of the profession, whether the important question, the dissimilarity or non-identity of syphilis and gonorrhœa, be not pathologically established. If this has been done, the object of this paper has been attained. If it has not, it will form the subject of a more extended investigation at an early opportunity.

ART. III.—*Notes on the Climate and Diseases of the City of Cork.* By JOHN POPHAM, A.B., M.B., Physician to the Cork North Infirmary. (Dublin Journal of Medical Sciences, Volume Fifteenth, p. 290. May 1853.)

The city of Cork lies in an irregular valley, which is open to the east and west, but is confined by hills at the north and south sides. On the west it is connected with the fertile valley of the River Lee, which, rising from the secluded mountain lake of Gougane Barra, so well known for its romantic scenery, flows in a devious course through a country of unexampled beauty until it reaches the city of Cork, where, receiving a large tidal accession, it is rendered navigable to its mouth for vessels of considerable burden. When the Lee is about a mile west of the city it divides into two channels which gradually diverge, but re-unite again, thereby insulating a considerable tract, upon which the main part of the city is built. This tract is not free from the defects natural to its low position, and being permeated by some small streams from the main branches, inundations occur when the river is swollen by heavy rains and high tides, especially when the latter are driven in by an easterly wind. From this cause the city anciently derived its not very flattering name; Corcagh signifying a morass. Before the marshes were drained, the city was small, occupying chiefly the present north and south suburbs, so that when Dr Gerard Boate, physician to Cromwell's army, published in 1642 his Natural History of Ireland, he assigns the *town* of Cork but the fifth or sixth place, ranking it with Londonderry, and after Dublin, Galway, Waterford, and Limerick.[1] Since then art has remedied many of these disadvantages, and places within the arms of the river have been rendered eligible sites for streets, some of them, strangely enough, retaining their popular name of Marshes. The smaller inosculating channels have been arched over; the river has been confined by handsome quays to its two divisions, and the main branches have been deepened, so that the tide rises to an elevation varying from sixteen to twenty-one feet at the bridges, thus allowing the large Channel steamers to load and unload in the midst of the city. Most of these changes have been effected within the last century.

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[1] Boate's History of Ireland, p. 6.

The excellence of Cork harbour for its security, and the facilities for trade which it affords, have produced slow but certain effects. Still, regarding Smith's estimate in his History of Cork as correct, namely, that the city trebled itself in the forty years preceding 1749,[1] when he wrote his work, and taking his return of 7366 houses in 1732, with a probable population of seven to a family, or 55,769 persons;—the ratio of the progress of Cork in size and population does not seem, according to the last census, to have been proportionate.

If, however, we take up Smith's map of the city, published in 1750, we shall obtain juster notions; the whole aspect of Cork, with the exception of the churches, having become so changed, that without these landmarks it could now be scarcely recognised.

The insular extent embraced by the north and south channels being fully occupied, presents limits rather detrimental to the commercial development of the city. There are no sites, either, for handsome squares, such as are seen in other cities. The rising grounds, therefore, north and south of the river have been availed of for terraces and villas, delightfully open to the sun, and affording many of the advantages of a more genial climate; several of these are occupied by merchants, who convert their shops or warehouses in the flat of the city to purposes of business. That part of the northern hill which is to the west of the city constitutes the delightful locality of Sunday's Well, the Montpelier of Cork as it was called, once so famous for the health and amusements of the citizens, and now covered to the water's brink with seats and gardens. The continuation of this hill below the city, and leading to Glanmire, has also been largely built on, though the houses have not much uniformity; so that, viewed from the river, it looks like a distant city, and may be justly named New Cork. These picturesque terraces, forming the background to the charming scenery of the Cork river, have added much to its effect, giving the grouping and colouring of art to the graceful outline of nature. The northern hills swell abruptly in some parts from the river, and are fringed to the top with trees, and spotted with villas, some on a scale of almost princely magnificence. The green richness of the plantations in summer, and their mellow tints in autumn, afford views which are unrivalled, and make the fame of the Cork river no idle boast. The road leading to Glanmire skirts the margin of the river for nearly three miles, and forms a most agreeable resort. The river runs due east for this distance, and at Blackrock Castle it bends to a south-east direction, forming the grand expanse of Lough Mahon. Some of the beauty of the river near the city has been sacrificed to utility. It used to spread itself over a large space, looking, when the tide was full, like an extensive inland lake; but the channel became choked with the alluvial deposit rolled down by the current, and required a large amount of labour and expense to keep it navigable. An embankment was thrown out for the space of a mile, separating the bed of the river from the part covered by the tide; and this ground has been lately reclaimed and set apart for a city park, along which the Passage Railway winds its course.

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[1] Smith's Cork, vol. i., p. 401.

The *physical features* of the vicinity of Cork require a few observations. A valuable paper has been written by Mr Weaver on the Geological Relations of the South of Ireland. He observes, that high mountain chains commence in the western borders of Kerry, and extend thence in an eastern direction to the county of Cork, between the Rivers Lee and Blackwater, giving the picturesque appearance which distinguishes the scenery of these rivers. These chains are not so continuous as to present an unbroken range, but send off various lateral elevations, running nearly parallel to them, and intersected by glens. Between the two principal parallel chains, wide and deep valleys lie in a longitudinal direction, and form the channels of streams and rivers. The height of these mountains in Kerry is considerable: that of Gurrane-Tuel, the highest point in Ireland, is 3410 feet above the sea-level; but this elevation declines gradually to a few hundred feet. The hills at the north and south of the River Lee form part of these mountain ranges.

The *geological character* varies at different sides of the river. The north side consists of graywacké and slate; the sandstone is of a deep red and coarse structure; it is in parts very hard and difficult to be worked, but varies from the hardest grit to clayey rubble. In boring the tunnel of the Great Southern and Western Railway, much greater difficulty was experienced in working this than was expected. It was curious, that the very great hardness frequently yielded to the chemical influence of the atmosphere,—some kinds, when long exposed, becoming totally disintegrated. This graywacké formation extends along Glanmire. An isolated limestone strip, about two miles long, is found at Blarney, containing but few organic remains.[1]

The southern hill of the Lee consists of limestone of the transition series, and forms a continuation of a band varying in width from five miles to half a mile. It commences easterly at Youghal Bay, occupies the smaller islands of Cork harbour, then appears at the peninsula of Blackrock, and continues to keep the southern bank of the Lee until its junction with the Bride, when it leaves the former river, and keeping to both sides of the Bride for a short distance, ceases at the left bank, but continues at the right to Castlemore Castle, fifteen miles from Cork. This is flanked by by more or less elevated ridges of graywacké and slaty rocks, with which it interstratifies at their confines. The angle in which the strata are inclined to the horizon approaches at times near the vertical, but is chiefly between 70° and 80°, dipping to the south.

The organic remains of the Cork limestone were mostly determined by the careful scrutiny of the late Mr Samuel Wright of Cork, aided by Mr Sowerby. He derived them from the quarries of Evergreen, close by the city, and Ballinlough, on the south of the Lee. Of Invertebrata there are found;—of the crustacea, some undescribed trilobites; of mollusca, cephalopoda, comprising some orthoceratites and nautili; of conchiferæ, various species of productæ and spiriferæ, also some polyparia and crinoidea. Traces of Vertebrata also have been found. Casts of the vertebræ of fish were discovered by Miller. In the Blackrock quarries some beautiful specimens of amethyst have been dug up. Wavellite has long been found at Tracton Abbey, a few miles from this city, in sufficient quantity to supply the London mineralogical market; and, during the last year, a very pure silica, fitted for the manufacture of the choicest porcelain, has been discovered at Rostellan, on the eastern side of the harbour.

The difference of the soil at both sides of the river was noticed by Smith. " The north side," he says, " being coarse and hilly, is not so well cultivated as the opposite, which, being a fine limestone soil, affords plentiful crops to the industrious farmer.[2] The earth of the north side is mostly a yellow, gritty mould, which requires lime. Clay for bricks and potteries is found in abundance near Douglas, two miles from Cork.

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[1] Transactions of the Geological Society of London, Second Series, vol. v.: " On the Geological Relations of the South of Ireland," by Thomas Weaver, Esq., F.R.S.

[2] Smith's Cork, vol. i., p. 354.

In the vicinity of the city, the numerous valleys which lie parallel to the hilly ranges, or cross them, are frequently overflowed, forming large bogs. In the basin of the Lee, to the west of the city, a vast marshy tract exists, extending to the very margin of the city, and in winter, when much rain has fallen, lying submerged for days and even weeks. In very high floods, the Dyke Walk and houses are inundated. Another of these large boggy tracts extends for miles between Blarney and Cork; and at the south side a deep and almost impassable morass, called the Ballyphehane bog, reaches from Evergreen to Douglas, and is crossed by the Bandon Railway, in forming which peculiar engineering difficulties were experienced from the yielding nature of the soil. Besides these fresh-water marshy tracts, there are others to the east of the city partly saline, being exposed to the tides; to them belongs the ground lately reclaimed from the river. The constant evaporation from these large humid surfaces must keep up a peculiar hygrometric condition of the atmosphere.

Indeed, the city of Cork has obtained a degree of *notoriety for rain* much more than it deserves. A Cork mist, like a Killarney shower, is a common jest with travellers. This charge of perpetual humidity, brought against the climate, may certainly receive some countenance from the miry streets, which strike female tourists with dismay. It is, unfortunately, a question as yet unsettled, though affording scope every year for much civic eloquence, whether it is the duty of the private citizens, or of the public commissioners, to keep our *trottoirs* clean.

On making a careful examination of the registry of the weather, taken at the Royal Cork Institution by Mr John Humphreys, the Librarian, I find that the average annual fall of rain for twenty-five years, to the end of 1852, as determined by the pluviometer, amounts to 38·096 inches; this high average being caused by the six years from 1846 to 1851, both inclusive, in which it rose to the very large amount of 51·542 inches. For the twelve years ending in 1839, the annual average did not exceed 34·212 inches. The extremes of variation are very great,—from 25·438 inches in the year 1844, to the unprecedented quantity of 60·375 in 1848. The difference of rain, in inches, between the years 1845 and 1846 is very remarkable, there being an increase of twenty inches in the latter, which was the year of the potato blight; the rains in 1845 being 32·783, and in 1846, 52·553 inches. From 1846 it continued to increase to 60 inches in 1848,—a period in the annals of this country terribly distinguished for famine and fever, from which year it has declined to

37·653 in 1852,—affording thus a rational expectation of the return of the seasons to their ordinary average. It is curious, that the end of the year 1852, in which so much rain fell in other places, was not characterised in this part of the country by an amount more than usual, though the number of rainy days was considerable. In the month of May 1844, we find the least monthly amount only 0·144; and in December 1847, the greatest, being 16·171 inches; in January 1851, 14·561 inches fell. In dividing the annual quantity of rain by the seasons, we find, in the year 1852, for winter (including the months of December, January, and February), 12·832 inches; for spring, 5·413; for summer, 8·030; for autumn, 11·372; the quantity of rain during the winter season is rarely less than one-third of the whole annual amount, and usually exceeds it; the least quantity is in the spring.

In calculating the fall of rain in Cork, I wish to state, that the Royal Cork Institution, where it was estimated, lies on the borders of the river, in the flat of the city, and the rain-gauge is kept at a height of about twenty feet above the sea-level. Recent observations have elicited the curious fact, that nothing is more variable than the quantity of rain falling in places near each other in horizontal distance, but of different degrees of elevation, local causes exercising a considerable influence. This variation was found by Colonel Portlock to exist in Cork. In an interesting paper, read before the Cuvierian Society, the quantities noted at the Cork Institution and the barracks, in the year 1848, were respectively 48·613, and 33·410 inches,—thus making the amount which fell in the flat of the city one-third greater than at the barracks, which are built at a considerable elevation above the city, but less than half a mile in horizontal distance. "The elevation of the barracks places them," he observes, "in a drier stratum of air, and the form of the hill on which they stand allows air charged with moisture to spread easily and descend." Whether this reasoning be founded on correct principles, I cannot say; but it is curious that a conclusion opposite to it is arrived at by Mr Miller, who gives, in a paper printed in the Philosophical Transactions for 1849, the results of his interesting experiments on the fall of rain at various heights in the lake district of Cumberland and Westmoreland. His theory is, that the degree of humidity increases with the elevation, arriving at its maximum at about 2000 feet above the level of the sea.[1]

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[1] Philosophical Transactions, vol. cxxxix., p. 73.

The number of rainy days in Cork is not so great as might be anticipated from the annual amount of rain. In the register kept at the Cork Institution, the fall of the one-fiftieth part of an inch is considered to constitute a rainy day. Of these the number during twelve years, from 1841 to the end of 1852, gives the yearly average of 134, which is less than in many other localities. The extremes are indeed very distant, varying from 96 in 1843, to 172 in 1848, the latter number being in correspondence with the large amount of over 60 inches which fell in that year. Distributed among the seasons, the average of the twelve years gives for winter, 39 days; for spring, 30; for summer, 29; for autumn, 36. The maximum monthly number of wet days was 25 in January 1851; the minimum, one day in May 1844. The month of January afforded the greatest average number of rainy days, the mean of twelve years being fifteen days. The months of June and July gave the minimum of about nine days. The months of October, November, and December of 1852, and January of this year, have been unprecedented for the number of rainy days, which amounted to 80 in the four months.

But though the mean number of rainy days is comparatively small, the amount of variable weather is considerable, there being an average of 70 days in which showers, mists, or fogs occur, but not yielding an amount of water appreciable by the rain-gauge. Snow is rarely seen more frequently than three or four times a year; in the month of February of this year, when Dublin was visited by such heavy falls, Cork had it very lightly. Frost occurs about twenty-four times in the course of the year. The average of seven years, from 1846 to 1852 inclusive, gave but twenty-four nights in which the self-registering thermometer descended to 32°, or below it. In the order of the years, the numbers were 34 for 1846; 29 for 1847; 39 for 1848; 14 for 1849; 24 for 1850; 19 for 1851; and 10 for 1852. Hail rarely falls more frequently than five or six times in the year; on May 9, 1851, hailstones fell equal in diameter to the $\frac{7}{16}$ths of an inch; but this storm was general in Ireland, having done great mischief in Dublin. Thunder and lightning sometimes happen but once, seldom more frequently than four times in the year. Meteoric phenomena are very unusual; a note in the registry on September 4, 1851, states that a funnel-shaped cloud, like a waterspout, approached at 12 o'clock at noon very near the ground, rain following two hours after. We possess little information of any exactness upon the state of the climate before our own time. In Dr Roger's Essay on the Epidemic Diseases of the City of Cork, published in 1734, we find some tables of the weather, but too loosely noted to be much depended on. For twelve months from 1st May 1721 to 30th April 1722, his observations were the following:—Rain, 80 days; mist and showery, 119; fair, 139; snow, 4; hail, 3; days not given, 20.[1]

The *variability of the atmosphere in density* is such as may be expected in so changeable a climate. The results of the barometric tables for the seven years between 1st January 1845 and 31st December 1851, are the following:—The highest point of elevation which the barometer reached in that period was on February 11, 1849, when it rose to 30° 88′; the lowest point of depression for the same period was on January 15, 1851, when the mercury fell to 28° 61′; thus giving for the seven years an absolute range of 2° 27′. The mean of the absolute maximums of *each* of the seven years was 30° 60′; the mean of their absolute minimums was 28° 71′, yielding thus a mean absolute range of 1° 89′, and differing by 38′ from the total absolute. The mean of the highest monthly pressure for seven years was 30·37°, the mean of the lowest, 29·27°; the mean annual pressure, 29·85°. The monthly range for seven years gave for January an interval of 1° 33′ between its highest and lowest points; for February, 1·17°; March, 1·33°; April, 1·13°; May, 1·07°; June, 0·94°; July, 0·86°; August, 0·81°; September, 1·18°; October, 1·24°; November, 1·43°; December, 1·41°. We find thus, that the months of November and December afford the greatest range, while those of July and August afford the least. The mean of the whole is 1·16°. The time when the pressure was greatest occurred in the autumn and winter months, and the times of least pressure at variable periods in the same seasons, the extremes approximating in spring and summer.

The *comparative frequency of particular winds* is an important element in climatic descriptions, especially in their bearing on health and disease. In examining the registry, the great preponderance of the westerly winds over the easterly is remarkable. I have computed the duration of each wind for nine years,—viz. from 1844 to 1852, both inclusive, and have found that the average annual constancy of each at a given period of the day was the following, taken in order of frequency:—The north-west, the most constant, blew for 75 days; the west, for 72; south-west, 63; south-east, 37; north, 32; east, 30; north-east, 28; south, 27. As to the prevailing wind of each month it is very variable. In the months of January and February, the north-west, west, and south-west, contend for predominance, the former more frequently recurring than the others. In March it sometimes happens that the north-west blows for a continuance, but it is varied by a run of easterly and north-easterly gales, these occasionally, but not always, being the prevailing winds. In April the north-west blows for about seven days, the east and south-east sometimes taking the lead, and more rarely the westerly. In May the south-west prevails, but in some years, biting easterly gales occur. In June and July the south-west and west are the chief, the latter blowing about eight days. In August we again meet the north-west, diversified with the west and south-west. In September the easterly breezes again make their appearance, the three points of east, north-east, and south-east, often occupying more than half the month. In October the prevalence is disputed by the easterly points and the north-west, but the latter is the more usual. In November the westerly winds blow frequently, but sometimes in this month, and in December, northerly winds blow for several days in succession; the north-west is usually the chief. The south wind is the least frequent of all our winds, seldom blowing in any month more than four or five times, and preferring, contrary to what is popularly supposed, the colder to the warmer seasons.

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[1] Essay on the Epidemic Diseases of Cork, by J. Rogers, M.D.

The *mean annual temperature* of Cork for the three last years amounts to 51·5°; distributed among the seasons, we have for winter a mean of 42·4°; for spring, 50·6°; for summer, 62·3°, and for autumn, 50·5°. The mean temperature of each month is as follows:—January, 40·2°; February, 44·3°; March, 46·3°; April, 51·3°; May, 55·3°; June, 61·3°; July, 63·8°; August, 62°; September, 58·6°; October, 50·2°; November, 43°; December, 42·8°. The difference of the mean temperature of the winter and summer seasons is 19·9°, and of January and July, the coldest and warmest month, is 23·6°. The difference of the mean temperature of the successive seasons is, of winter and spring, 8·2°; of spring and summer, 11·7°; of summer and autumn, 11·8°; and of autumn and winter, 8·1°; shewing that the transition from the extremes of temperature is gradual and not sudden. This is still further shewn by the difference of the mean temperature of the successive months. Thus, between January and February we have a difference of 4·1°; between February and March, of 2°; March and April, 5°; April and May, 5°; May and June, 6°; June and July, 2·5°; July and August, 1·8°; August and September, 3·4°; September and October, 8·4°; October and November, 7·2°; November and December, 0·2°. Here the maximum differences are at the period of transition of spring to summer, and the months of autumn. The mean monthly range for three years gives us 27° as the interval in January, between the maximum and minimum temperatures of 53° and 26°; in February, 21°, between the extremes of 55° and 34°; in March, 21°, between 57° and 36°; in April, 28°, between 64° and 38°; May, 31°, between 71° and 40°; June 26°, between 75° and 49°; July, 28°, between 78° and 50°; August, 28°, between 76° and 48°; September, 24°, between 72° and 48°; October, 28°, between 64° and 36°; November, 22°, between 59° and 37°; December, 27°, between 56° and 29°.

The absolute range of the thermometer must be greater in proportion to the length of the period examined up to certain limits, but, estimating it during seven years, we can fairly conclude that we have very nearly reached the highest and lowest extremes which the mercury undergoes in this part of Ireland. The extremes, as given by the self-registering thermometer, were, for the highest degree, 84° on 14th June 1846, and the lowest, 20° on 17th January 1850; thus giving an absolute range of 64°. This range does not, however, occur in any one year. For instance, in the year 1850, with its lowest extreme of 20°, the absolute range was 59°; in 1851 the range was 54°; and in 1852 it was 53°. Thus it would not give an accurate estimate of this subject to judge by so small a period as three years.

The daily range of temperature in the several months, for the twenty-four hours, gives for January the maximum variation of 21°, and the minimum of 7°; for February, 19° and 4°; for March, 20° and 5°; for April, 26° and 7°: May, 27° and 12°; June, 22° and 11°; July, 23° and 8°; August, 20° and 10°; September, 22° and 8°; October, 19° and 6°; November, 23° and 6°; and December, 18° and 6°.

The general appearance of a country is to be received as evidence of its climate, and the effects of the open air on *vegetation*, especially on that of the less hardy species of plants, allow us to form probable conclusions as to its influence on animal life and comfort. The aspect of our fields and gardens testifies amply to the mildness of the climate of the south of Ireland. If our fields are less luxuriant in summer than in less equable climates, they are in winter clothed with a perpetual green. The myrtle thrives in the open air with us almost as freely as in Portugal; though we are told by Meyen,[1] "that it perishes when so exposed in Berlin," which is nearly in the same latitude as Cork. The moisture of our climate, and the constant presence of clouds, prevent the atmosphere from being so much heated in the day, and cooled in the night, as in the drier and more transparent air of inland countries, thus affording a more equable temperature. It must, however, be remembered, that the physiognomy of a country depends not merely on conditions of climate, such as winds, heat, moisture, and the like, but on artificial culture; and the higher degree of cultivation usual in the neighbourhood of larger cities, if not allowed for, might mislead us. We have surer data to proceed on in the bloom of our gardens, and the number and nature of our indigenous plants. It is rare, indeed, to find a winter so severe with us as not to provide a bouquet for the florist; and one of our local celebrities[1] presented the Cork Cuvierian Society with a December wreath of forty-seven flowers, culled from his parterre, grown, with little protection, in the open air, and many of them regarded as tender plants in the parts of Ireland north of Munster. With respect to our indigenous botany, I refer, with much pleasure, to the Flora of Cork, published by Dr Power.[2] According to his catalogue, the number of plants in the county of Cork amounts to 1820; of which 885 are phænogamic, and 935 cryptogamic. This list has been much extended since its publication in 1845, and patient research would doubtless extend it still more. It is still below the number found in higher latitudes, Denmark having 1034 phænogamous, and 2300 cryptogamic plants.[3] The great majority of our plants are to be found in a radius of a few miles from this city. The Nymphæa adorns the still waters of the far-famed Blarney Lake, with its spreading leaves and beautiful white flowers just rising above the level of the water. The *Pinguicula grandiflora* is found near the city. The *Trichomanes speciosum*, the elegant Bristle fern, has been met along the Lee, that classic ground of the botanist. Among the "plantæ rariores" may be mentioned, the *Medicago maculata, Trifolium arvense, Dianthus deltoides*, &c.; but it would occupy us too much to give any further illustrations.

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[1] Geography of Plants, by F. J. Meyen, M.D., Professor of Botany in the University of Berlin. Ray Society, p. 13.

Any thorough inquiry into the *sanitary state* of Cork should embrace a number of considerations. We must premise that this city was formerly the great depot of the navy provision trade, and though something is still done in this line, the adversity with which the whole country has been overwhelmed, and the many rivals which free trade has encouraged, have rendered it no longer her staple export. In the north outlets of the city were situated the abattoirs, and the ill consequences to health resulting from these slaughter-houses have been often deprecated. The lanes and alleys along the old Blarney road were and are still the depositories of the offal in every state of putrefaction, and the hand of reform is yet unraised to abate this Augean nuisance. Hence, though this suburb is delightfully situated for health and enjoyment, it is disgraced by every species of contamination; and whatever progress the rest of the city may have made, this portion continues,—as it was described by Dr Rogers in 1720,—uncleansed, and by consequence unhealthy. In every epidemic we find it severely visited, and in the late fatal epidemic fever it supplied the hospitals with most of their patients.

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[1] Botanical Illustrations of Climate, a paper read before the Cork Cuvierian Society, by Alderman Dowden.

[2] Fauna and Flora of the County of Cork, by Drs Harvey and Power, and Mr John Humphreys.

[3] Meyen, p. 3.

Another impediment to the progress of sanitary measures arises from the influx of peasants from the rural districts resorting to the city for relief, or for emigration. The latter classes of persons coming to Cork for embarkation, and having little surplus money, flock into the cheap lodging-houses, already centres of contagious disease, and thus spread the evil. No limit can satisfy the cupidity of the owners of these miserable abodes. Some of them are cellars, with no possible ventilation; while in others, more favourably circumstanced, the crowding together of numbers in a space vastly disproportioned, produces like dangerous effects to health. Hence these houses have been long notorious as dens of moral and physical disease, and the public voice has often called, but with little effect, for a remedy. Whether the new Lodging-house Act will meet the evil, remains to be proved.

Another cause, promoting an insalubrious state of the city, is found in the intra-urban interments. The graveyards of the churches are of very limited space, and, being nearly all situated in the centre of poor and populous parts of the city, they cannot but exercise an injurious influence. One of the chief medical establishments of the city, the North Infirmary, is built on the skirts of a graveyard, and, independently of the depressing effects naturally arising from the associations of such a place on the imaginations of the sick, a still greater evil arises from the malarious influence resulting from the continual dredging up the unctuous soil for fresh interments. This was found so great a nuisance during the late fever epidemic, that the General Board of Health had to command the graveyard to be closed against new burials. It is to be regretted that the vested rights of individuals, and the want, though urgent, of a suitable cemetery without the city, prevent the redress of this grievance.

In one essential article of health, *good water*, a great advance has been made of late years. The waters of the Lee, brackish from the tide, were formerly in use among the poor, and caused various abdominal diseases. Public fountains have been lately constructed in all the populous districts, affording a sufficient supply. The water used by the more affluent citizens is particularly free from impurity. An analysis of the pipe-water made by Professor Davy, of the Royal Dublin Society, yielded only 7·20 grains of solid matter in the imperial gallon, being in the proportion of $\frac{1}{9745}$ to the whole, chiefly consisting of the carbonates of lime and iron, but in quantity so small as not to interfere with common use. When we compare this amount with the quantity of solid matter in the water supplied to London, the purest of which contains 13½ grains, or double the amount, we have reason to be content with our local superiority in this important auxiliary to health.

Before speaking of the *endemic diseases* of Cork, we may say a few words on the epidemics which have, from time to time, visited the city. A general history of epidemics is still a desideratum in medical literature; but the recent establishment of the Epidemiological Society of London promises to do much in this wide field. It is probable that, in the writings of the old annalists, such contagious maladies as were rapid and deadly in their effects were called "plagues," otherwise we must suppose our western isles to be more frequently visited by oriental pestilences than is at all probable. The earliest of these epochs is the year 548[1], when we are told that "Munster was afflicted with a great plague." Whether this was the terrible pestilence which depopulated the earth in the reign of Justinian, A. D. 542, lasting for fifty-two years, and so graphically described by Gibbon,[2] we cannot say. The years 664 and 685 witnessed also fatal plagues. Recurrences of these pestilences are chronicled as occurring in 1348, 1361, 1370, and 1383. These dates correspond with the four great epidemics of the "Black Death," that terrible pestilence of the fourteenth century, which desolated the known world. "Ireland," says Hecker, "was much less heavily visited than England."[1] The next epidemic pestilence noticed in our annals, occurred in 1504; but whether this was the petechial fever which then, for the first time, began to attract notice in Spain and Italy, it is difficult to say. In 1528 we find it recorded that "a malignant disease, called the sweating sickness, raged in Cork."[2] We may take the opportunity here of correcting an error into which Hecker falls, in stating that this hidrotic disease "did not extend westward to Ireland."[3] In the British Isles, he observes, neither Irish nor Scotch suffered; and he endeavours to explain it by some peculiarity in the English constitution, or "their gluttony and rude mode of life!"[4] He concludes hastily that historians are silent about the extension of so calamitous an event to Ireland.

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[1] Bede, Hist. Eccles., lib. ii., cap. xxvii.

[2] Decline and Fall of the Roman Empire, chap. xliii.

In the years 1535, 1547, and 1607, dreadful visitations of pestilences are recorded, of the nature of which we can form no idea. In 1650, plague is again noted, probably the epidemic dysentery, so fatal during Cromwell's visit to Ireland. From this to 1708 we have no account of any epidemic; but the last date, and that of 1718, are assigned by Dr Rogers, in his work on the Diseases of Cork, as remarkable for typhus fever and malignant dysentery. The same author cites 1731 as a fatal year from smallpox, and 1740–41 from fever and dysentery. The years 1762 and 1782 were notable for a widespread and fatal epidemic of influenza. The last year of the past century was a famine year, and the present century opened disastrously with famine, and its not uncommon sequela, typhus fever. Over 4000 persons, writes Dr Milner Barry,[5] were attacked in Cork, of the poor; so that, in 1802, it was judged necessary to open a district fever hospital in the city.

In 1803 another very severe outbreak of influenza occurred, and in 1810 contagious fever became again general; great destitution existing among the poor. This returned, in 1814, in a mitigated form; but a scanty harvest occurring in 1816, typhus broke out the following year, and over 8000 cases of it were met with in Cork and its vicinity.[6] This epidemic prevailed all over Ireland, and also in Great Britain; and in 1818, a Bill passed the Legislature to establish Fever Hospitals in Ireland. In 1822 and 1826, a return of

[1] Hecker's Epidemics of the Middle Ages. Sydenham Society Ed., p. 27.

[2] Vide Smith's Cork, vol. ii. p. 35.

[3] Hecker's Epidemics of the Middle Ages. Sydenham Society Ed., p. 247.

[4] Ibid., page 294.

[5] Report of Cork Fever Hospital, in the Edinburgh Medical and Surgical Journal, vol. xiv. p. 117.

[6] Practical Observations on Typhus Fever, by A. Callanan, M.D.

fever took place in Cork, as well as other parts of Ireland, and has been described by the able pen of Dr Graves.[1] In 1832 a new pestilence visited us in the form of that fearful epidemic, the Asiatic cholera, in its slow but certain progress through the world. In 1837, one of the most fatal and universal epidemics of influenza within our memories occurred, followed the next year by an outburst of fever. In 1841 and 1842, a violent epizootic swept off thousands of cattle; and in 1846 and 1847 occurred that fatal period in our history, when, to the miseries of want and despair, from the failure of the potato crop, were added the ravages of a widely diffused and mortal epidemic of typhus fever. In March 1849, we were again visited by the cholera. But since this period our country shews some sign of recovery from her almost exhausted vitality, and general health and public prosperity seem settling on a more stable foundation.

Such is a succinct account of the chief epidemics occurring in the city of Cork from the earliest record to the present time; and there are few countries in the world where these terrible national scourges commit such havoc as in ours, from the improvident recklessness of the people. The celebrated historian, Niebuhr, has alluded, with his usual acumen, to the singular influence which fatal epidemics exercise over the character of nations, whole periods of history being explained by their appearance or disappearance. All great epochs of moral degradation are connected, according to him, with great epidemics. In the succeeding generation every thing perishes, and barbarism returns. His observations have much truth, for when men get reckless of life they care little for the means of preserving it; and when, on the other hand, the fear of death absorbs all the thoughts, the framework of society is broken. To trace the effects of these epidemics in our own countrymen would afford a painful interest. Ireland, in the late epidemic, seemed like some "city of the plague" in the middle ages. Terror was in every breast, the dark robe of mourning was seen in every family, and country districts, once populous, now appalled the heart by their loneliness. Hordes of human beings, enfeebled by sickness and attenuated by want, rushed into cities for an asylum, meeting their fate with a resignation like the apathy of despair.

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[1] See his Clinical Lectures, edited by Neligan, vol. i. Lecture vii. The Irish School of Medicine has, alas! to deplore the recent loss of this accomplished physician, one of its brightest luminaries. "Quando ullum inveniet parem?"

Of *endemic diseases* we may begin with Fever, as always lurking in this city, liable to be drawn out on any violation of the laws regulating health. It would almost appear that the Irish constitution has a greater affinity for fever than that of other nations, as no sanitary measures can wholly extinguish this disease, which smoulders on until some spark kindles it into a flame. It follows the Irish labourer even to other countries, and Glasgow and Liverpool trace their fevers to the quarters of the Irish, and cast on them the stigma of originating the disease. It must be admitted that most of the stimuli which quicken the seeds of fever exist among the lower classes of Irish,—deficient and bad food, intemperate habits, neglect of personal cleanliness, and the *effluvia accumulated from numbers herding together, like gregarious animals, in ill-ventilated dwellings.* These hurtful habits the Irishman does not leave behind him when he emigrates. The evil effects of over-crowding were fatally evident from the pressure on the different workhouses during the famine, shewing that, of all the auxiliaries to health, pure air is that which can be least neglected with impunity. It is to be feared that, unless due precautions be taken, the practice which prevails in this city and elsewhere, of congregating hundreds of young females in industrial establishments, where confined space and a not healthy occupation are combined, will lead to evils which are likely to lessen the utility of these institutions.

Among diseases which appear to travel in cycles, *Intermittent Fever* may be included. It was, as might be expected from the marshy nature of the district about Cork, at one time one of our most common affections, and "the fever and ague" were as familiar words in this city as in the swamps of the Mississippi. It is curious that, without any obvious cause, this disease, for a number of years, totally disappeared among us, so that during my connection with the North Infirmary, as physician, the only instances I witnessed for more than ten years, occurred among sailors coming to this port in corn vessels from Galatz, and other places along the Danube; and some of our medical pupils passed their noviciate without witnessing a case of the disease. The first indications of its return were seen in March 1850; but the persons attacked were from particular localities. It continued till July and August, when it disappeared until the March of the following year. Males were much more frequently affected than females. No age escaped, from the infant at the breast to the threescore and ten, but chiefly it seized on persons in the prime of life. Some of the cases came from the high and dry grounds and the sandstone formations, such as the Fair Hill; others from the humid soil and limestone of Evergreen and Blackrock.

Many of the lanes in the suburbs had it, and persons residing along the river quays did not escape, though it was capricious in its selections. In the Cork Union Workhouse, situated near a bog, a number of cases occurred, and the districts of Killeen, Whitechurch, and Ballincollig, were largely attacked. The type was chiefly tertian; some cases of quotidian were met; quartan was rare. It was very amenable to quina; but unless the remedy was long continued, the disease relapsed.

Another disease, once frequent in Cork, then disappearing almost totally, and lately returning with great severity, is *Dysentery.* In our annals, dysentery held the second place for fatality among the "*leaguer sicknesses*" of Ireland. In 1796 it committed great ravages in the ranks of the Irish Brigade quartered in Cork; it was an autumnal disease, and followed generally in the wake of fever. It continued for a few years after; but the next account of it, as an epidemic, is to be found in Dr Cheyne's valuable paper in the Dublin Hospital Reports.[1] According to the late Dr Milner Barry, of this city, it occurred among the poor chiefly after a wet summer, and carried off one in every three attacked. It was supposed by Dr Barry to have been caused by bad water; and he states that in the old Cork Barracks the troops had been supplied with brackish water from the Lee, and were attacked with dysentery, but Surgeon Bell had a supply of pure spring water brought to the Barracks, which caused the disease to disappear. The decline of dysentery in Cork was, perhaps, too hastily concluded by Dr Barry to be due to the erection of public fountains, and a better supply of good water. It certainly so far declined, that for a number of years it had scarcely an existence in the city. In a statistical report of the diseases admitted into the North Infirmary, during five years from 1838 to 1843, which I drew up for the Medical Section of the British Association, at its Cork meeting, I found that but seven cases of this diseases had been received into the hospital.[2] But the potato failure in 1846 and 1847 brought back the disease in all its virulence, and in a highly contagious form, being united with continued fever. Since the cessation of the epidemic, dysentery has remained in this city as an endemic, though, as we have seen, the want of good water is no longer a cause. The dietary of workhouses has certainly a tendency to promote the disease, and it is one of the chief causes of death in these institutions, chiefly among the very young, and the aged and infirm. Of 1101 deaths which occurred in the Cork Union Workhouse, from February 1852 to February 1853, 131, or one-ninth, were caused by dysentery, besides those cases where it complicated and hastened the issue of other diseases. But it would be wrong to conclude that all these persons were first attacked while in the workhouse; a great number were brought in from the electoral districts in a hopeless state. The change of diet has been too sudden and too absolute for the poor; they do not understand the mode of preparing the Indian meal for food, and have not the means of cooking it properly. Besides, they are obliged to use an article of inferior quality.

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[1] Dublin Hospital Reports, vol. iii. p. 11.

[2] Report of the British Association for the Advancement of Science, for 1843, p. 84.

Another disease resuscitated after the potato failure was *Scurvy*. The disease of higher latitudes had become almost unknown until the year 1847; the only cases in our hospitals being of sea scurvy among sailors, chiefly of guano vessels, the continued exposure to the vapour of ammonia in these ships appearing to act as a determining cause. We could assign no obvious cause for the epidemic of land scurvy; in some the excessive use of tea or coffee was assigned by them, but this is no rare abuse. It prevailed very much in the workhouse, and attacked females more than males,—individuals, moreover, frequently in the prime of life and strength. The gums were generally first affected, and then stiffness of the joints came on, and pains of the legs, and the symptoms declined in the same order. This disease has been so well described by the late Dr Curran, in this Journal,[1] that I have nothing to add to his observations. The epidemic passed away as suddenly as it came, and has not since returned.

Diseases of the Chest hold a prominent place in our catalogue, as they do in other parts of Ireland. Of these, bronchitis senilis and phthisis are the most important. The results of hospital experience afford ample proof that the latter of these diseases is not constant in its prevalence, but of an intercurrent character, long periods sometimes occurring which are comparatively free from it. Our cases of very acute phthisis are rare, the disease seeming very much prolonged with us, especially among the higher classes, who can guard against undue exposure to weather. Of the 1101 deaths from all diseases in the workhouse, during the past year, 57 were from consumption.

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[1] Dublin Quarterly Journal, N. S., vol. iv., p. 83.

The *diseases peculiar to workhouses* would form an interesting inquiry. In every epidemic the first cases usually occur there, so that the physicians of these institutions can see before others the coming storm. Some diseases may be called indigenous,—such is that fruitful cause of death which is called in workhouses the *house-decline*,—a kind of cachexia nosocomiensis,—in which the patient, chiefly a child, droops day by day, with no pain, no expression of regret or repining, and but little evident disease. Another of these diseases is *purulent ophthalmia*, which commits terrible havoc among all ages, but chiefly the young, running its course with great rapidity. It is a disease of the end of spring and the summer, and seems more closely connected with foul air in heated wards than with other causes,—ablutions with impure water also help to produce it. In some cases it could be traced to lime rubbed to the eyes, or the juice of the *euphorbia helioscopia* dropped into them, by children who did not wish to be sent to an auxiliary workhouse away from their mothers.

Of *diseases rarely met with* in Cork may be mentioned calculus of the urinary bladder. Very few cases of this disease present themselves at the hospitals, though the operations which have taken place have been very successful. Bronchocele is another unfrequent disease, considering the marshy nature of the country. Colica pictorum is a disease almost unknown. I recollect of but one case of wrist-drop from this cause. Delirium tremens has declined with us of late years, though the enthusiasm of the advocates of total abstinence has abated. It has been observed, that when temperance societies are in full operation, outbreaks of this disease are met, which are unusual at ordinary times. The drunkard then throws off all shame, and drinks deep rather than taste not. The chief advantage of these societies seems to be, that they have struck at that monstrous evil which prevailed formerly among all classes of society in Ireland, which made drinking to excess a popular and venial indulgence. It is a remarkable trait of the Irish character that, under the late pressing evils of destitution and sickness, *suicide* has been so rare. The unhappy outcast has lain down in the streets or fields to die, perfectly resigned to his fate, but never seeking to hasten it. The sense of injury, much more than the feeling of privation, hurries on the Irish peasant to commit crimes; and to take away the life of another is but a light matter compared with the act of shortening his own. They have truly little thought for the morrow; hope is rarely extinct, and their agricultural habits and feelings of religion keep them from yielding to sudden acts of despair.

I have to say, in conclusion, that in reviewing this paper I am sensible of many omissions and defects which I cannot now rectify. I have in the Tables followed the mode adopted by Sir James Clarke, in his excellent work on Climate. I have endeavoured to do, upon a small scale, for the city of Cork, what Dr Scott has so ably done for Queenstown.[1] The results only of long and troublesome Tables have been given, as the calculations would be tiresome. Their accuracy, however, can be depended on. Any person who has made similar calculations well knows the exhausting trouble attendant on them, and will, I feel certain, make allowance for many deficiencies.

ART. IV.—*Case of Inflammation of the Brain, terminating fatally. Pericarditis; Gastritis.* By EDWARD D. DE VITRE', M.D., Physician to the Lancaster Dispensary and House of Recovery.

Mr C., an architect, aged 46, a strong, though spare man, who by intense application and perseverance, from being a common stone-mason, ultimately attained a high degree of proficiency in his profession, of temperate habits, and who had previously enjoyed excellent health, with the exception of being occasionally subject to a disordered state of the bowels, is the subject of the following case.

May 4, 1833.—I was requested to meet in consultation along with Mr Foxcroft, who had charge of the case for some time previously. Mr C. was then labouring under the symptoms of chronic *bronchitis;* cough troublesome, mucous expectoration, breathing not much hurried, pulse 145, soft and regular, skin hot, thirst, anorexia, and bowels slightly constipated. He complained of no pain whatever, even on violent coughing, but experienced a constant degree of constriction across the lower part of the sternum, and occasionally slight palpitations of the heart. The posterior portion of the left side was a little dull on percussion, and the only stethoscopic sign indicative of thoracic disease, consisted in the mucous rattle which was very audible down the side of the spine; notwithstanding, the respiratory murmur was not extinct. A slight degree of dulness on contraction of the auricles was the only sign furnished on exploring the heart.

A large blister was applied over the lower part of the sternum; aperient medicine exhibited; and a pill composed of ipecacuan, digitalis, and hyoscyamus, ordered to be taken three times a day.

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[1] Dublin Journal of Medical Science, O. S., vol. xiii., p. 55.

5th.—The patient was much relieved; the pulse was 120; he had slept tolerably well; the bowels had been freely opened, alvine evacuations dark; the blister was discharging copiously. Cont. Pil.

It is unnecessary to give a farther minute detail of the treatment. Suffice it to say, that after this he gradually continued to improve, the constriction at the sternum was overcome by the frequent repetition of blisters, particular attention was paid to the state of his bowels, which were frequently very obstinate, and with the aid of an hydrocyanic acid draught at bed-time, his cough and all bronchitic symptoms vanished. In the beginning of June, he was enabled, though much reduced in strength, to resume his usual occupations. But his pulse was still rapid, not being less than 100; in character as above. He, however, insisted on this being *constitutional.* I may state, that from some symptoms presented during his convalescence, we had reason to suspect the presence of worms; but the administration of anthelmintics failing to expel any, and the symptoms disappearing, we abandoned the idea.

The preceding part of Mr C.'s history may to some appear supererogatory; nor would I have related it, had it not been for the purpose of considering it in connection with the fully developed symptoms of diseased action in the brain, which, it will be seen, so speedily succeeded.

In the first week in August, while attending a daughter of Mr C.'s in typhus fever, along with Mr F., in reply to our inquiries respecting his own health, he stated that "he felt tolerably well, but that he had lately experienced an occasional involuntary twitching of the right upper eyelid." He made no other complaint. He slept well; his appetite was remarkably good; and he had in a great measure regained his strength. The bowels were slightly constipated, pulse 110, and a little harder and fuller than formerly.

Some cathartic medicine was ordered, from which he obtained relief.

On the 13th of August, I was sent for in great haste, in the absence of Mr Foxcroft, the messenger stating that Mr C. "had taken a fit." I found the patient assuming an air of indifference, though evidently much alarmed, with great anxiety of countenance, vacant stare, eyes glistening. The pulse was 125, regular, full and hard; tongue coated with a white fur. He related that whilst in the act of drawing a plan, without any previous warning, he suddenly dropped the compasses ; that, at the same time, he experienced considerable convulsive action of the muscles of the right cheek, and was unable to speak. He had taken some *brandy*, which he fancied had revived him. I saw him not more than twenty minutes after the attack, when all symptoms of such an attack had disappeared, with the exception of slight *thickness* in expressing words.

I now, for the first time, apprehended the presence of disease in the brain, and had him cupped on the nape of the neck, to the extent of fourteen or fifteen ounces. The bowels were freely evacuated, cold evaporating lotions were applied to the head, and rest and low diet enjoined.

Next day, he declared himself well, but was occasionally experiencing creeping sensations on his right cheek, commencing on the gums of the lower jaw of that side, traversing the cheek and terminating on the side of the head; with a similar feeling along the right arm, and a tingling at the extremities of his fingers. The pulse was 120, softer, tongue cleaner, and countenance more composed. The alvine evacuations were dark and fetid, and containing small white lumps in size and appearance resembling a blanched almond. The cathartic medicine was continued.

His symptoms rapidly disappeared, and he was resuming the duties of his profession, contrary to our entreaties, when on the 22d, whilst accompanied by a friend in a gig, he was suddenly seized with a violent convulsive paroxysm of the muscles of the right cheek, total loss of speech, and paralysis of the right arm. Being in the immediate neighbourhood of the County Lunatic Asylum, my friend, Mr Davidson, the highly intelligent Surgeon to the Institution, instantly repaired to the spot, and had him cupped on the nape of the neck, and stated in a note to me his conviction of the nature of Mr C.'s affection. Our patient partially recovered the use of his speech and arm in about half an hour, and on his return home sixteen leeches were applied to his temples; a seton was introduced in his neck, and the cold lotions re-applied to his head.

After this period, he was strictly forbidden to take any active part in his profession, and everything likely to produce mental excitement was carefully avoided. A very slight indistinctness in pronunciation continued, his tongue inclined a little to the right side, and he did not regain the free use of his right arm. His appetite was good, he slept well, and declared himself free from all pain, his pulse varying from 100 to 130, but the creeping sensations soon extended to both cheeks, and the tingling at the extremities of the fingers to the left hand. He was well aware of an approaching convulsive paroxysm, from the coldness of his fingers, and the blanched appearance of the nails; an unfortunate circumstance, as it tended to keep his mind in a perpetual state of excitement and alarm.

On the 5th of September, he was seen by an eminent physician from London, who thought all his symptoms might be ascribed to the presence of *Taeniœ*, or some other source of irritation in the digestive tube, and at his suggestion, anthelmintics of various kinds were exhibited, but without any better result than formerly. Slight convulsive paroxysms occurred at intervals, without aggravating his symptoms much, till the night of the 30th, when he was attacked in bed with one, severer than usual, which for several hours deprived him of the power of speech, rendered his right arm paralytic, and his left slightly affected.

Ten or twelve ounces of blood were abstracted from the temporal artery, and the cold shower-bath was ordered to be used night and morning. Under this treatment he again gradually recovered his speech and the use of his arms, till the 16th of October, when another paroxysm was attended with the same results as before. After this he experienced prickling sensations in both his legs, but particularly in the left. A convulsive attack on the 27th, deprived him of speech, and the use of all his extremities; the right arm and left leg were most paralysed. Very little improvement took place till the 29th, when another attack rendered him more helpless than ever, and in two days afterwards, he could not articulate any word beyond a monosyllable. The pulse was 125, small and soft; the extremities were cold. Small doses of strychnia were given in form of pill, three times a day, increasing the quantity gradually.

On the 2d November he was much improved. On the 5th he spoke with very little faltering, and repeated the alphabet without a mistake, excepting at the letter *w*. He walked firmly, and could use both his arms pretty freely, pulse 110, bowels regular. On the 6th, 10 A.M., as yesterday, 6 P.M., speech was very indistinct, and his extremities more powerless. No convulsive attack, however, took place. There was however some vomiting; the pulse was rapid and weak. On the 7th he was much worse. Vomiting in the afternoon was distressing, with acute pain over the region of the stomach. The pulse in the evening imperceptible; there was clamminess of the skin, and the patient was evidently sinking. He lingered till 6 A.M. of the 8th, when he expired.

I may remark, that during the whole of his illness he had no headache, and that his intellect never seemed much obscured. If we made any erroneous statement relative to his own profession particularly, he gave evident tokens of his anxiety to put us right, even when he could not articulate a word. When he attempted to speak, he appeared suddenly to forget himself. But that his memory was defective, he would not allow, ascribing it solely to his inability to pronounce ; and he nodded assent when we supplied him correctly with what he wanted to express. He was not so irritable nor so childish as patients under such an affection generally are, but shewed an extreme degree of anxiety about his recovery.

The body was inspected thirty-six hours after death by Mr Foxcroft, in presence of his assistant and myself.

On opening the head, the *dura mater* was found præternaturally adherent to the bone, but without appearance of inflammation. The vessels of the *pia mater* over the posterior part of the left hemisphere appeared gorged with blood. On slicing the brain, a very extensive ramollissement of the whole posterior right[1] lobe of the *cerebrum* was found, extending to the middle lobe down to the *centrum ovale.* In the centre of this softening there was a circular portion rather larger in circumference than half-a-crown, of a liver colour, with a vascular rim, and softer in texture than the cream-coloured mass in which it was imbedded. The lateral ventricles contained about one ounce of clear serum. The right hemisphere of the brain was perfectly sound. The *cerebellum* was free from disease.

Miliary tubercles were discovered in various parts of the lungs, but most abundant in the posterior part of the left lung, where were two old cicatrices ; no tubercles in an advanced state ; no adhesions. On cutting into the lungs a quantity of frothy mucus escaped. The bronchia were partially dilated, and the lining membrane was pale. A few ounces of serum were contained in the cavity of each pleura.

The heart was found closely adherent throughout to the *pericardium,* the adhesions evidently of old formation, with the exception of one or two points, where a softer medium of union existed, depending on a recent effusion of violet-coloured lymph. The heart was small in size, of a pale colour, as if macerated in water, and firm in texture ; in other respects normal.

The stomach presented a number of highly vascular patches on its mucous coat, particularly on its lower surface, and around the pylorus, extending to the commencement of the duodenum ; and the lining membrane was in a softened state.

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[1] This should be *left* lobe or *left* hemisphere. ED.

The rest of the viscera were healthy. About one pound of serum was contained within in the cavity of the peritoneum.

The spine was not examined.

Inflammation in the brain, I believe, is only to be subdued by adopting active depletory measures early. But in an insidious case, such as the above, it is more than probable that the disease is far in advance before its presence is detected by the medical man; nay, even in many cases before the patient himself makes any complaint. The stethoscopic signs of pulmonary disease detected at my first attendance on Mr C. were certainly insufficient to account for all his symptoms; and I have now little doubt that even then inflammation existed in the brain, and had probably been present for some time previous, and that depletory treatment to some extent at that time might have been of the greatest benefit. Yet he had no pain in his head, no creeping sensations then, no symptoms, in fact, at all referable to such an affection, excepting the state of his pulse, which was equally indicative of disease in some other tissue. Under the treatment then adopted all his symptoms yielded, with the exception of the state of the pulse; and this was even considerably better.

Ramollissement once established, I more than doubt the efficacy of bleeding. It may, indeed, arrest the further progress of inflammatory action; but it can have no more beneficial influence over the softened brain than over the contents of a pulmonary abscess. Pathological research may yet furnish us with some definite diagnostic signs of inflammation in the brain. But I strongly apprehend that, when creeping sensations, and tingling at the extremities of the fingers, along with convulsive action, are once unequivocally established, they are indicative of the product of inflammation rather than of inflammation itself.

The existence of *pericarditis* was not suspected. The patient never complained of any pain in the region of the heart, nor had symptoms referable to a disease of that organ or pericardium, excepting when in May he experienced constriction at the lower part of the sternum, and occasional, very slight, palpitations; enough, however, to induce me to use the stethoscope. In the various examinations made, I certainly never did detect any thing like a bruit or fremissement, or any other abnormal sign, beyond the dulness already mentioned.

Respecting the attack of gastritis, I shall offer no observations. The most ardent admirer of Broussais could not have desired any other termination.

ART. V.—*Statistics of the Mortality by Shipwreck in the British Navy for Fifty-Seven Years, from* 1793 *to* 1849. By THOMAS STRATTON, M.D. Edinen., Surgeon, Royal Navy. (In a Letter to Sir William Burnett, M.D., K.C.H., K.C.B., Naval Medical Director-General).

In Mr Gilly's *Narratives of some of the Shipwrecks of the Royal Navy* (Second Edition, 1851), there is given in an appendix an alphabetical list of all the ships wrecked since 1793; the number of the crew of each ship, and the number of lives lost is mentioned, except in some instances where the number of the crew or the number lost cannot now be ascertained. I have reckoned the particulars for each year, and have placed in the same line the number of seamen and marines voted for that year. The numbers voted for each year from 1793 to 1820 are taken from James's *Naval History*, and in a few instances from Brenton's *Naval History*. From 1821 downwards the details are taken from Hansard's *Debates*. In some years the numbers voted differ for parts of the year; thus, in 1840, there were 24,165 seamen voted for the year, and also 2000 for ten months, and 2500 for three months; these additional numbers I have distributed in their proportion over the thirteen lunar months.

Besides the ascertained or enumerated loss, there are also many instances where it is now impossible to ascertain how many casualties occurred. It will also be remembered, that this account does not include deaths from drowning happening to individuals or to boats' crews:—

In the 57 years there were wrecked 407 ships, being an average of seven wrecks a-year; 27 is the greatest number of wrecks in one year; in thirteen years no wreck occurred.

Of the 407 wrecks there are 29 where the number of lives lost is not recorded; by the remaining 378 wrecks, 11,537 lives were lost, being an average of 30 and a fraction for each wreck.

The greatest number of lives lost in one year was 2274; the *recorded* mortality in 57 years is 11,537, or an average of 202 lives a-year.

Of the 407 ships there are 37 where the number of the crews is not noted; in the remaining 370 the total number of the crews is 52,633, being an average complement or crew of 142 to each ship.

In the 407 ships there were 66 where all on board were lost, being one ship in every six.

In two hundred and seventeen (217) wrecks no lives were lost.

To 52,633, the recorded crews of 370 ships, we add 5254 as the estimated crews of the remaining 37 ships whose crews are not recorded (at the average of 142 to each), making 57,887 as the total crews of the 407 ships.

To 11,537, the mortality by 378 wrecks, where the number lost is noted, we add 870, the estimated mortality by 29 wrecks, where the loss of life has not been ascertained (at the average of 30 to each), making 12,407 as the estimated mortality by 407 wrecks.

Dividing 57,887, the total crews, by 12,407, the total mortality, we have one death in every 4⅔.

Dividing 12,407, the total mortality, by 57, the number of years, gives 217⅔ as the average yearly number of lives lost *by shipwreck*, and exclusive of other deaths by drowning, as by falling overboard, and by boat accidents. On referring to the excellent work of Dr Bryson, I find that on the African station, in the course of 21 years, and among a total force of 20,604, there were 135 deaths from accident; and of these 76 were from drowning, being 3·6 per thousand. There were, no doubt, other deaths from drowning, but 76 are all that are distinctly noted. (*On the Climate and Diseases of the African Station.* Clowes: London, 1847. P. 147).

I have as yet considered the shipwreck mortality in reference only to the total numbers on board wrecked ships. There is farther to be considered, the proportion which the mortality bears to the combined strength of the Royal Navy and Royal Marines; a glance at the column for the total strength will give some idea of this proportion. In some years the number of boys is included among the seamen. It is to be understood that under the head *seamen* are included naval officers, and under the head *marines* the officers of that indispensable arm of the maritime force of Great Britain. In considering what proportion of the Royal Marines are exposed to sea risk, I believe I am correct in supposing that about two-thirds of the force may be viewed as embarked.

I am not aware of any previous examination in a minutely statistical way of the life-risk incurred by those who go down to the sea in ships; and I hope that the present contribution on the subject of maritime vital statistics will be interesting in a general way, and also to life-insurance companies, who, when imperfectly informed as to the risk they are to encounter, are apt to err on the side most safe for themselves, and to charge more than is necessary for the assurance.

PRINCE EDWARD ISLAND,
April 1853.

Year.	Seamen.	Boys.	Marines.	Seamen and Marines. Total.	No. of Ships lost.	Enumerated Loss of Life.	Additional Loss.
1793	36,000		9,000	45,000	1	10	
1794	72,885		12,115	85,000	4	125	
1795	85,000		15,000	100,000	5	97	Unknown of *Diomede*, crew 294; and *Musquito*, crew 50.
1796	92,000		18,000	110,000	17	190	Greater part of *Amphion*, crew 215 and unknown of *Vancean*, crew 60.
1797	100,000		20,000	120,000	17	286	All of *Viper*, crew unknown; unknown of *Fortune*, crew 86
1798	100,000		20,000	120,000	13	35	
1799	100,000		20,000	120,000	12	570	
1800	100,000		20,000	120,000	6	355	Unknown of *Galgo*, crew unknown.
1801	100,000		20,000	120,000	11	231	Some of *Leopard*, crew unknown.
1802	26,000		22,000	44,000	3	121	
1803	60,800		19,200	80,000	9	491	
1804	78,000		22,000	100,000	18	72	Some of *Venerable*, crew 590.
1805	90,000		30,000	120,000	11	123	
1806	91,000		29,000	120,000	9	747	
1807	98,000		31,400	129,400	27	1643	All of *Java*, crew unknown; unknown of *Snipe*, crew 50; all except four of *Firefly*, crew unknown.
1808	98,600		31,400	130,000	25	360	Some of *Banterer*, crew 155; unknown of *Delight*, crew 95.
1809	98,600		31,400	130,000	15	245	All of *Contest*, crew unknown; unknown of *Sealark*, crew 20.
1810	113,600		31,400	105,000	12	580	Unknown of *Monkey*, crew 50.
1811	113,600		31,400	145,000	17	2274	Unknown of *Thistle*, crew 50.
1812	113,600		31,400	145,000	19	310	Unknown of *Encounter*, crew 60.
1813	108,600		31,400	140,000	11	65	Unknown of *Bold*, crew 55; unknown of *Woolwich*, crew 135.
1814	82,000		23,700	105,700	10	160	Unknown of *Anacreon*, crew 120; and of *Dart*, crew 40; some of *Leopard*, crew unknown.
1815	66,538		18,846	85,384	3	236	Unknown of *Dominica*, crew unknown; some of *Penelope*, crew 284.
1816	24,000		9,000	33,000	6	1	
1817	13,000		6,000	19,000	4	127	
1818	14,000		6,000	20,000	0	0	
1819	14,000		6,000	20,000	0	0	
1820	15,000		8,000	23,000	1	19	
1821	14,000		8,000	22,000	1	0	All of *Bermuda*, crew unknown.
1822	13,000		8,000	21,000	2	0	Unknown of *Confiance*, crew 100; some of *Drake*, crew 76.
1823	16,300		8,700	25,000	1	100	
1824	20,000		9,000	29,000	0	0	
1825	20,000		9,000	29,000	2	0	
1826	21,000		9,000	30,000	1	75	
1827	21,000		9,000	30,000	3	32	
1828	21,000		9,000	30,000	4	197	
1829	21,000		9,000	30,000	2	34	
1830	20,000		9,000	29,000	1	0	
1831	22,000		10,000	32,000	0	0	
1832	18,000		9,000	27,000	1	52	
1833	18,000		9,000	27,000	0	0	
1834	17,500	1000	9,000	27,500	1	35	
1835	15,500	2000	9,000	26,500	0	0	
1836	22,700	2000	9,000	33,700	0	0	
1837	23,165	2000	9,000	34,165	0	0	
1838	23,165	2000	9,000	34,165	1	33	
1839	23,165	2000	9,000	34,165	1	0	
1840	25,703	2000	9,000	34,703	0	0	
1841	30,500	2000	10,500	43,000	1	0	
1842	30,500	2000	10,500	43,000	1	0	
1843	26,500	2000	10,500	39,000	1	0	
1844	23,500	2000	10,500	36,000	0	0	
1845	27,500	2000	10,500	40,000	1	0	
1846	27,500	2000	10,500	40,000	0	0	
1847	27,500	2000	11,460	40,960	2	0	
1848	27,500	2000	13,500	43,000	0	0	
1849	26,000	2000	11,000	39,000	0	0	

ART. VI.—*New Views on the Physiology and Pathology of Arteries.* By J. M. COTTLE, L.R.C.P.

The subject of the physiology of arteries has commanded the researches of the best physiologists from the time of Harvey to the present period. A large majority of these, down to Alison, have maintained that the arteries exercise an active power in propelling the blood. From the date of the publication of the Supplement to his Physiology by Dr Alison, the opinions of physiologists on the subject have changed. This has been occasioned by the difficulty of detecting active contraction in arteries during the pulsations of the heart, in consequence of the reaction of physical elasticity occurring at the same moment; and the fact, also become apparent, that the muscular contraction of arteries with which we are most familiar,—that produced by experiments on the living body by foreign stimuli and cold,—partakes so much of the nature of passive muscular contraction, as to come under the designation of *tonicity,* and consequently, to render it obviously unfit for propelling onwards the blood. Such contraction being slow and continuous, and being directed upon the column of blood when the aortic valves are open as well as when they are closed, it was evident, must antagonise the contracting force of the ventricles.

Since this was pointed out by the latter author, arteries have presented an anomaly in physiology. While the other two systems of bloodvessels are acknowledged to aid the circulation, no aid is supposed to be afforded by arteries; and the muscular fibres of these vessels have presented an exception to other muscular fibres; for while arteries possess the acknowledged mechanism for active as well as passive contraction, in common with other muscular fibres, no active contraction is assigned to them. The subject has been felt to be a difficulty, which the latter view regarding it has only increased. A long array of facts, supplied by the normal and abnormal condition of arteries, pointed out by the elder physiologists, and conclusions drawn from experiment, and which bear no other interpretation than a local action in arteries propelling the blood, bear testimony against the conclusion now commonly adopted, and call for a revision of the question. The objects of this paper are to demonstrate the operation of an active contraction in arteries, synchronising with the diastole of the heart, and to apply this, and certain other facts in physiology, in explanation of the phenomena that bloodvessels exhibit, locally, in the states termed *excited nutrition* and inflammation, and generally to the physiology and pathology of arteries.

The grounds on which my arguments shall be formed are, that there exist in the muscular tissue of arteries (as in all other muscular tissues of the body), either two distinct contractile forces, or two distinct manifestations of one contractile force; that, in the ordinary state of the body, the two forces or manifestations have their respective states in arteries and other muscles. This I would term the *status par* of each force or form of contraction, to distinguish it from the extraordinary and subordinary states that they assume under certain conditions of the body, both in health and disease; and these latter states of the contractile forces I would designate by the terms *plus* and *minus*. If I succeed in maintaining that there exist in arteries two such forces, or two manifestations of one force, and that their respective states are governed by the same laws as the other forces of animal life, I shall be enabled to reconcile the various theories that authors have held in regard to the condition of bloodvessels in excited nutrition and inflammation,—the *simplex atonia* of Hoffmann, for instance, the *solus spasmus* of Cullen, the *active dilatation* of Hunter, the *vital sensibility* of Graves, and the views of John Thomson, Wilson, Philip, and Hastings,—altogether constituting a subject which Travers has pronounced to be the foundation of medical and surgical science; and Professor Paget (whom I take for the most recent expression of the general opinion) says, the causes of such phenomena (referring to the different velocities of blood) he is unable to explain.[1] If, however, I succeed only in shewing, that there is but one of the forms of contraction in arteries, and I establish the fact as to which of these forms or forces the contraction belongs, and shew the laws by which it is governed, I shall be enabled to throw much light on the present subject.

The physiology of the active and passive forces of muscle will first claim attention, in consequence of the difference in the ideas that are prevalent respecting them, and also that those adopted by the author may be known to the reader. There is a doubt whether the forces referred to are really separate entities existing or generated in muscle or not. Irritability, or the active force, is usually so considered; and the passive, or Tonicity, is generally spoken of as a variety of contractility,—another manifestation of the force irritability,—and therefore a quality of a body or thing, rather than the thing itself. It will, however, be more convenient, as well as more consistent with truth, as I suppose, to treat them as distinct and separate entities of the nature of the nerve-forces, the great cosmic bodies, or electricity, and consequently taking their places amongst the dynamic forces of animal life. The exact truth cannot be determined. But if such a view is ultimately found in error, a certain present advantage will have accrued from it; for, like Mr Locke's classification of ideas, although it was ultimately found defective in fact, it might serve to simplify and make intelligible an otherwise abstruse subject.

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[1] Lectures on Inflammation; printed in Rankine's Half-yearly Abstract, p. 292.

I am aware that some of our first physiologists incline to the opinion that the two forms of contractility are but different manifestations proceeding from the same force. I would observe that this question is not in any way essential to the doctrines which I wish to propound in this paper. I shall maintain that the two forms of contraction have their distinct and separate states in muscular tissue; and I conceive that the same laws which regulate these contractions in relation to each other, are no other than the laws that regulate the different manifestations of *vis nervosa*, about which the same difference of opinion may be fairly held.

The grounds on which my hypothesis is based,—that they are distinct and separate dynamic forces of animal life, are, that they may be respectively distinguished; *1st*, by their modes of action; *2d*, By a difference of the stimuli that excites their contraction; *3d*, By the pathological states proper to each; and, *4th*, By a difference in their therapeutics.

The term Tone (*Tonus*) was applied by Stahl,[1] to the motor faculty, which is characterised by a slow contraction, unaccompanied with the sensible oscillations or intermissions which distinguish active muscular contraction or irritability. Blumenbach and Tiedemann termed the same Tonicity. This form of Contractility is exhibited in a tendency to contraction, which is almost, though not altogether constant in muscular tissue during animal life, and continues in the body to near the commencement of molecular death, or decomposition, as seen in *rigor mortis;* the contraction of divided muscles and arteries during amputations, is a familiar display of its effects. The muscular contraction which bears the name of irritability, is an energetic and transient shortening of muscular fibres which is excited by stimuli, natural and artificial; and, according to my view of its action, it is energetic, and transient in both kinds of muscular fibre, with this difference, that in the non-striated it is less energetic; and though answering rapidly to its natural stimulus, it is slow and tardy under artificial stimulus.

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[1] Dissertatio de motu tonico vitali. Jenae, 1692-4.

The name was given by Glisson; but both the terms Tonicity and Irritability are inconvenient, so far as they are expressions of the prominent qualities of the forces, and divert the mind from the idea of the abstract and original nature of the forces themselves, and also that the word Irritability has different meanings in physiology, psychology, and ethics. For these reasons, Haller endeavoured to change the term Irritability to *Vis insita;* and as the necessity for the change has greatly increased since his time, I propose to substitute the terms *Vis passiva musculosa,* and *Vis activa musculosa,* for the two respectively. The difference in the stimuli that excite the action of the two forces, is observable in the fact, that one of the most powerful sedatives with which we are acquainted,—cold,—and which acts as such, on the nerve-forces, and the active muscle-force, operates as a powerful stimulant to the passive muscle-force.

The sedative effect of a freezing temperature applied to a muscle separated from the body immediately after death, annihilates Irritability altogether, but exerts no effect upon Tonicity, except to delay its occurrence until the muscle is unfrozen; and the distinctness of the two forces is farther evidenced by the fact that Tonicity commonly exists in the body during the period of somatic death, after irritability has departed, as seen in *rigor mortis.* Tonicity is rapidly developed when a muscle is immersed in water, or water is injected into its substances. I account for this by a mechanical movement that is effected by the water amongst the red corpuscles of the blood lodged in the capillaries. By means of endosmosis, the oxygen in the corpuscle is pushed on and brought into contact with new portions of the fibres, which is an essential condition of muscular contraction; and I would suggest that it is by a movement which is known to take place in the capillaries after death that the same condition is fulfilled, and *rigor mortis* is produced; the stimulus which calls it into action being possibly also supplied amongst the obscure products, that are known to be the first fruits of decomposition. *Rigor mortis* is purely tonicity, its relations only being different.

It is asserted that the manifestations of Irritability are different in the two kinds of muscular fibre. It is probable, however, as I shall presently shew, that this assertion is without foundation. The evidence of passive contraction being excited by a higher temperature than that of the blood, has rested on experiments which consist of the immersion of frogs and portions of muscles separated from the body in hot water. Mr Hunter's well-known experiments on this subject were so conducted; and it is important to observe, that the contraction that is the result, is not the effect of the water absorption to which I have alluded, as may be easily proved by subjecting a portion of muscle severed from a body recently dead, to the influence of hot air, by placing it in an oven; by which means it is almost immediately thrown into a state of rigidity. A temperature a few degrees above the ordinary temperature of the blood appears to act as a stimulus of the active muscle-force. This is shewn by the increased force of the pulsations of the heart and arteries that is occasioned by a high temperature of the body, or by the effect of cold in allaying the same, when applied either generally or locally to the body. A still higher temperature I have noticed is a stimulus for the passive force. The changes which take place in the bloodvessels in the web of the frog's foot, as seen under the microscope, when stimulated by the needle, and first fully described by Professor Paget, affords, as I view it, an instance of the states of the two muscle-forces alternating and succeeding each other in regular ordination. This takes place in accordance with a law in physiology, that whenever one of the forces of animal life is augmented, or in excess in the body, the other forces become subordinate in their action. A similar law Hippocrates speaks of as applicable in pathology;[1] and it has a familiar illustration in the fact, that when the brain is engaged in realising perceptions which are derived from the action of one organ of sense, it often happens that sensations, the product of the other organs of sense, do not become perceptions.

When the point of a needle is drawn slightly across the artery and vein, usually seen in the side margins of the web of a frog's foot, they *slowly* contract (after the manner that the divided ends of arteries contract in amputations, or in the same gradual way that experimenters have described larger arteries to contract under artificial stimulus in the mesenteries of toads, &c., &c.); and after remaining contracted for a few minutes, they as gradually enlarge to proportions exceeding their natural dimensions. The mode of contraction thus exhibited is essentially Tonicity, or the passive force, inasmuch as it cannot be likened to the active force, however much or in whatever set of muscles that force may be modified or made slower by the adjunctive quality of rhythm. In this enlarged state of the vessel will be recognised the Dilatation of Excited Nutrition, and inflammation; and it is observable that the renewed application of the needle to an artery in this enlarged state produces scarcely any farther contraction; and what little is seen soon disappears. This, I presume, is in consequence of the other form of contractility, or the active force having taken possession of the field. Tonicity is *minus*; the vessel having lost the support of a permanent contraction, dilates with the pressure of the blood, and in accordance with a law in physiology, the walls become hypertrophied; and hence is supplied the machinery for a greater development of either force afterwards, as the case may be. A probability that in this enlarged condition the passive force is absent and the active force present, will depend on the proofs that I shall advance, for the existence of a transient contraction or true irritability in arteries; the effects of which I shall afterwards more fully enlarge upon, when I attempt to explain the laws that govern the phenomena, that are observable in arteries during the states of excited nutrition and inflammation.

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[1] Δυο πὸνων ἆμα γινομενων, μή κατα τον αυτὸν, ὁ σφοδροτερος αμαυροῖ τον ἑτερον.—Aphor., lib. 2, No. 46.

In corroboration of the opinion that in the enlarged condition of arteries here spoken of, passive contraction is not present, or rather cannot be excited by ordinary means, I have ascertained, that muscles much inflamed are not at all liable to *rigor mortis*; and that muscles that are but slightly inflamed, remain flaccid at a period when the other muscles taken from the same animal are become stiffened, and that the stiffness that partially inflamed muscle does undergo, takes place after a much longer interval. The state of the arteries that is produced by the application of the point of a red-hot needle to the membrane of a bat's wing, furnishes, as I suppose, an instance of the effect of a high temperature in exciting the passive muscle-force. It is observed that the vessels immediately around the eschar,—that is to say, those that have not been injured by the burning, are thrown *slowly* into a state of contraction, by which they become closed, and remain so for twenty-four hours or so. This is the passive force. Immediately beyond this circle of contracted vessels, there are others that assume the enlarged condition referred to, which I shall argue denotes the absence of the passive force, and, as I shall afterwards shew, exhibit the signs of the presence of the transient or active contraction in operation in their walls.

The muscle-forces, correctly speaking, cannot suffer augmentation in the body; but certain materials may accumulate in the blood, whose destination it is to supply nutrition and growth to the mechanism for their development; and other materials may so accumulate which are appropriate to effect those changes in the gray vesicular nervous matter, which is the essential condition for supplying motor stimulus to the muscle-forces. It is not soproper to say, that the muscle forces exist in the body in direct compound ratio to these two sets of materials, as it is to say, that the aptness or cabability of the body for generating them exists in that proportion to those materials; and it is essential to a correct knowledge of the conditions of arteries,—a subject to be treated of in another part of this paper,—to bear in mind, that these materials bear no proportion to the amount of muscular contraction that may be excited; for the materials may be present in the blood, and but a small amount of contraction take place.

On the other hand, as often happens, the contractions may be *plus*, when the materials are *minus;* and may continue in excess until the body is exhausted by the loss of those materials. The progress of hydrophobia furnishes an instance in Pathology of active contractions in excess when the materials in the blood are *minus*. The advanced stage of Tetanus, again, is an instance of the passive contractions in excess, the materials being *minus*. These instances relate to the muscles generally. There are instances of the same disproportion which refer especially to the muscular tissue of arteries. The jerking pulse peculiar to hectic fever, and common to inflammation, is that of active contraction *plus*, and the materials *minus* in that tissue. The small pulse in the cold stages and collapse of fevers, &c., is that of the passive contraction *plus*, and the materials in the blood *minus*.

To prove the existence of the active muscle force in arteries, it is necessary to shew, in the first place, that there is present in their structure, the regular mechanism for its development; but this has been made so obvious by anatomists, as not to require demonstration in this place. Next in importance to this is the evidence of the fact that there is presented to the muscular structure, the proper motor stimulus natural to the other hollow muscles; and as the contraction excited by such stimulus would be unavailable for propelling the blood, unless operative exactly at the prediastolic period of the heart's action, it is necessary to shew that both these conditions are fulfilled. It is well known that the principal natural stimulus to muscular contraction in the muscles which belong to the functions of animal life, is volition; and that the principal stimuli that excite the muscles of organic life are certain impressions which go the round of a nervous arc in the following order. They are first transmitted by esodic nerve-fibres to the nerve centres of these movements, from whence is issued motor stimulus to the centres of the motor nerves of the muscles engaged in these functions.[1] The muscles concerned in deglutition are wholly, and those of respiration are principally, excited by such impressions. The foregoing are ruling principles in physiology in regard to the natural stimuli of muscles; and there is a principle also that all muscular contraction is independent of nervous systems, irrespective of the force of which it is the effect, or irrespective of the two kinds of muscular tissue (striated and non-striated) or the two classes of muscles (animal life and organic life), in which it is manifested; albeit muscular contraction is ruled and regulated, and in the two great instances just referred to, supplied with motor stimulus, by the nervous systems.

There is a group of muscles which appear to form an exception to those already spoken of in regard to motor stimulus; inasmuch as they do not derive their stimulus from nerves. These are the *hollow* muscles situated in the walls of tubes and cavities; their office is in the organic functions, and their fibre is generally, though not invariably of the unstriped kind. They exist in the bladder, the heart, the œsophagus, the intestinal canal, and the bronchial tubes; and their motor stimulus is derived from the *stretching* or *distension* of their muscular fibres.[2]

It has been shewn by Volkmann, that when the lungs have been removed from a body recently dead, and then stimulated by galvanism, the bronchial tubes contract with sufficient force to extinguish a candle by the expulsion of the contained air. I have therefore included these fibres in the foregoing list, as it is probable from hence, and more in unison with the physiology of other hollow muscles, that they have not their natural stimulus from the centre of respiration, but from the distension of their tubes. It will be recollected that the other respiratory muscles, when acting in the function of respiration, have their stimulus from the centre of respiration; and that when they are engaged in the functions of animal life, they derive their stimulus from volition. Since Mr Bowman ascertained by the microscope that a single muscular fibre insulated from the rest might be made fully and effectually to contract, there is, perhaps, no fact better settled in physiology, than that muscular contraction is independent of nervous systems. The question of the natural motor stimulus of the hollow muscles, is not so decidedly determined. The fact, however, that the organs in which these muscles are situated, will contract in their natural orderly manner by artificial stimulus, when removed from the body, makes it more than probable that their stimulus is derived from the source which I mention; and that what is peculiar to the contraction of each, in regard to order, or rhythm, is conferred by minute ganglia in the organs themselves. The contractions of the intestinal canal are especially of this class; it being kept in remembrance that a further control is exercised upon their movement by nerves, and that the actions of defecation, urination, &c., derive a stimulus from nerves.

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[1] The nervous arcs spoken of in the text are described in accordance to my suggestions for a better explanation of the mechanism of diastolic nervous action. See my *Manual of Physiology.* P. 167. London, Highley and Son.

[2] Todd and Bowman, *loc. cit.*, p. 173.

The motor stimulus which I claim for the active and transient contraction of arteries is the distension given to the walls of the tubes by the two ounces of blood that are thrown into the arteries by each ventricle during the systole of the heart. The same occurs at that particular division of the heart's action (the prediastolic period), which is the appropriate juncture for the action of a stimulus for signaling the arteries to contract during the diastole, thereby concentrating, the contracting force upon the column of blood at the moment when the closure of the aortic valves causes it to be pressed onwards towards the distal portions of the tubes, in conformity to a known law in hydraulics. Although the *rationale* of this application of the general law of the stimulus of hollow muscles to arteries is strikingly obvious, I am not aware that it has been ever so applied by physiologists. Whytt, who strenuously contended for a contracting power in arteries for propelling the blood, speaks of the *bulk* of the fluid as exciting to contraction the muscular coat;[1] but he does not speak of it as operating at that period, when alone it can be effective,—when the aortic valves are closed, nor as fulfilling the conditions of the other hollow muscles in regard to stimulus; and he mixes with his reasonings so much of the doctrine of stimulating substances in blood for exciting the same contraction, that no correct notions have ever been derived from his views.

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[1] Phys. Essays, p. 41. Edinburgh.

The question that is first suggested by the proposition that such an action exists in arteries, is, Why is not the energetic and transient contraction here contemplated visibly excited in arteries by experiment during life, by the application of foreign stimuli to their walls? There are several reasons why such effects should not be visible. The muscular fibres of the hollow muscles are grouped and designated by the tubes in which they are contained. The muscle-forces of these fibres have the same laws in all situations. Nevertheless, the laws are modified in certain districts; those of the intestinal tube, the œsophagus, the heart, and bladder, differ respectively in regard to modifications effected by nervous centres on their action either in regard to rhythm or their motor stimulus. I claim for arteries the same distinguishing peculiarities; and one of these furnishes a reason why active contraction, induced by foreign stimuli, is not very apparent. Tonicity has its peculiar import in the physiology of each of the groups of fibres alluded to; but in arteries there is a large series of effects, both in health and disease, that makes it probable that the force of passive contraction is more than usually prominent in the physiology of their muscular fibres. When, therefore, a foreign stimulus is applied to a living artery, both forces are excited together, according to known laws;[1] and, in accordance with the peculiarity of arteries just referred to, namely, that they usually exert more passive force than other hollow muscles, the passive force is excited in a greater ratio by the foreign stimulus; and as the effects of both forces are blended, that of the active or transient contraction is shrouded in the effects of the other. On the other hand, when the natural stimulus of distension operates upon arteries, the active force only is excited, distension not being known to be a stimulus of the passive force.

It is no objection to this hypothesis, that there are not in arteries any visible signs of dilatation during the slow contraction that follows foreign stimuli, such as we witness in muscles that are not situated in the walls of tubes. It must be recollected that "muscles have not the capacity of elongating themselves; when once contracted, they remain shortened, notwithstanding the contractile force has subsided, unless their ends be drawn apart by some extraneous force."[2] It is not meant to be affirmed, that the muscular fibres themselves remain in the shortened state that is essentially the contraction of muscle, but that a muscle may be in a shortened position, and yet the fibres themselves may not be in the state of contraction. The hollow muscles, it is well known, are but in a slight degree supplied with antagonistic force. When the walls of the intestinal tube contract upon their contents, they remain closed until a portion of matter is forced onwards to reopen them. But this is not entirely the case, for they are in a degree forced open by the yellow elastic fibre of their walls; and the same takes place in arteries when active contraction is carried farther than that diameter to which physical elasticity brings the vessel, when the muscular forces have forsaken the body after death.

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[1] The fact of the excitement of the passive force is shewn by the succeeding ends of arteries and muscles in surgical operations, and that of the active force by puncturing living muscles.

[2] Todd and Bowman, *loc. cit.*, p. 173.

Nor can it be said that the operation of the tonicity of arteries, which is so prominent in varying the size of arteries in conformity to the ever-changing phases of the blood, can interfere with the simultaneous development of active contraction; for it will be found greatly to illustrate the perfection of arterial action, that no mechanical law which may be brought into operation by the circumstance of the tubes varying their dimensions in accordance with the amounts of tonicity, or the varying volume of the blood, can interfere to obstruct the due operation of the active force.

The slow and gradual contraction that follows the application of foreign stimuli, or of cold, to arteries, has done much to establish the belief that there is no propelling power exercised by them upon the blood, seeing that such contraction cannot keep time with the diastole of the heart. Such contractions, however, display the characteristics of the passive force. They have their field in the economy of arteries, and fill an important page in their physiology, both locally and generally, both in health and disease; but the contraction is essentially that of tonicity or the passive muscle-force, as proved by its slow accession and remission from the vessel, and which is amply verified by the microscope, and by the contraction that follows on exposing an artery to the effects of cold in the living body. Such contraction is calculated to produce a great effect upon the volume of the blood circulating in arteries; whereas I shall presently shew that the sole mission, or office of the active muscle-force in one contraction (in its ordinary stasis upon arteries), is to assist in moving onwards (at a low computation), only one-fiftieth part of the blood which they contain.

The passive contraction in arteries, or that only which has hitherto been proved to exist in their structure, has been likened by some physiologists of the present day, to the contractions of the intestinal canal. There is, however, no analogy between the two movements. That of the intestinal canal is undoubtedly the active muscle-force, and although difficult to excite through nerves, the contractions are readily produced by stimuli directly applied; and although the movement when produced is slow in comparison with the active muscle-force, as otherwise exhibited, it will presently become apparent that all that is peculiar and different from the active muscle-force contraction is an added quality (rhythm) derived from the nervous system.

The main reason why we do not recognise the transient contraction of arteries, as it occurs during the diastole of the heart, exists in the fact that the reactive power of elasticity is in operation at the same moment. It should be borne in mind that the active contraction which I claim for arteries is but a force auxiliary to that of the ventricles; and that it is not requisite, and would be inconsistent with the physiology of the circulation of the blood, that any very sensible diminution of their tubes should take place during that contraction. The blood is not merely *en route* in these vessels;—it has functions to fulfil whilst there; and it has been ascertained by the experiments of Poiseuille, which are much relied on for their correctness, that the transverse contraction of arteries, during the diastole, is only one-twentieth of the diameter of the tubes; and this appears to be a greater diminution than might be expected, in proportion to the quantity of blood that leaves the arteries to enter the capillary system of vessels during the same period. The quantity that passes from the arteries (computed from that which enters) averages from one ounce and a half to two ounces at each contraction of the ventricles, and the whole amount of blood circulating in the arteries averages from five to eight pounds.

It is well known, that the principal alteration that is effected in the tubes of arteries by every injection of blood from the ventricles, is their elongation, whereby they became tortuous. It is obvious that a very rapid contraction must take place to produce the return of the vessels from these momentary curves, with which we are most of us familiar in watching the circulation of the blood; the longitudinal muscular fibres, and also the circular, which have been lately ascertained by Mr Dobie to take a spiral direction around the tubes, are calculated to take an active part in this movement. It is not to be expected that the transverse contraction of arteries can be recognised by the microscope during the diastole of the heart; but although not perceptible to ordinary measurement, it must be admitted that it is appreciated, and tells with all its force upon the comparatively small quantity of blood, that it helps to propel from the vessels.

It has been stated in one of our first works on physiology that there is no contraction in arteries that can help to impel the blood, for the reason that, if there were such contraction, it would not take place "simultaneously" at all points as the elastic coat does. It would, as in the œsophagus and intestines, act in successive portions, and the artery would, as in those tubes, be almost or altogether obliterated.[1] To establish the doctrine of the existence of a contraction in arteries that propels onwards the blood, it is necessary *inter alia*, to disprove what is stated in the foregoing extract. All that is peculiar in the contractions of the intestinal canal and œsophagus, is supposed to be conferred on the original properties of the muscular fibre of those parts, by the secondary influence of the nervous system. Such influence is termed Rhythm, and it is necessary to ascertain the point whether it is in accordance with the physiology of arteries that they should or should not be so endowed. Rhythm is supposed to be an adjunct quality imparted to muscles by the ganglionic nervous system; and the main reason for supposing it to be an added quality is the fact that it differs in every district or group of muscles, in which it is found. It does not determine the amount of contraction; for that exists, I have shewn, in compound ratio to the stimulus that excites, and the nutrition that generates the forces; but it modifies and fits the contraction in groups or districts of muscles to the purposes to which their action is destined. It is not a principle in physiology that Rhythm is an endowment of one or the other kind of muscular fibre; but it is common to the muscles of organic life, irrespective of their fibre. It is not seen in the muscles that are engaged in the animal functions; for whatever association in action belongs to the latter muscles is supposed to be effected by the cerebellum. It is worthy of remark, that as rhythm effects the bladder, the heart, and œsophagus, it appears to diminish nothing from the energy and transient character of the contraction, which I conceive to be the original characteristic of the unstriped as well as the striped muscular fibre. In the lower portion of the œsophagus, where the fibres are wholly unstriped, the contraction is rapid, energetic, and transient. The office of rhythm in the foregoing organs is to arrange the contraction in one portion of the muscular fibres of each organ, in fixed relationship, in point of time, to other portions of their fibres. In the functions of respiration and deglutition, the muscles engaged are endowed with rhythm, and they furnish instances of this quality regulating only the *time* and *order* of contraction of striated muscles without affecting their *energy*. The stomach and intestinal tube afford instances in which the natural energy and rapidity of the contraction of the non-striated fibre *is* modified by rhythm, so as to become slow, orderly, and partial.

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[1] Physiology, Anat. By Todd and Bowman. Part iv. p. 355.

It appears, then, that the general characteristics of Rhythm are so difficult of definition, and so dependent on the ends and purposes of the actions of muscles, that we are not justified in asserting that it is at all necessary to any law in physiology, that the muscular fibre of arteries should be endowed with it. On the other hand, there is good reason to believe that, as rhythm is made subservient to the local requirements of muscular fibres, and as in arteries it is not so required that it is not present in these tubes; more especially as it would only serve to encumber and fetter their action, and as it were throw out of gear that beautiful and perfect fitness for their action that their fibres possess in virtue of their unaided original properties.

It appears that the two muscle-forces as they exist in arteries differ in their natural stimuli. As regards the passive-force, it is plain that distension of the muscular fibre is not its stimulus; we are well aware how much the movements of arteries which are produced by the operation of the passive force are guided and controlled by the nervous systems. This is manifested by the increase or diminution of arterial action in particular districts; of which the flush of joy, the pallor of rage, the blush of modesty, the determinations of blood coexistent with the reproductive processes, and occasional increased formative powers in particular organs of secretion, are the indirect results. This series of effects is attributed to the ganglionic nervous system. Anatomy discloses the fact, that certain *fine* nerve fibres (not having the axis-cylinder contour) are peculiar to the coats of arteries, and to glands; and that these either originate in, or traverse the ganglia of the sympathetic. It teaches also that these same fibres enter the trunks of the cerebro-spinal nerves, and through them may be traced to every nerve-centre of the cerebro-spinal nervous system. What proportion of these fibres originate in the gray vesicles in the sympathetic, and what from the other centres, is not determined.

Such is the nervous mechanism on which is based our knowledge of the connection of arteries with the nervous system; and it is plain that the machinery is perfect for supplying motor stimulus and other nervous influence from any nervous centre of the body. I would suggest that not only are the movements of arteries of the passive kind controlled and governed by the emotional centres, which is generally admitted, but that the natural motor stimulus is supplied for these movements from the same source, or at least by nerves.

The grounds on which I found this conjecture are, first, that immediately after death the manifestations of tonicity, or the passive force, which through life gives a firmness to muscles, quit the body; notwithstanding the force itself has not departed, as is proved by its after recurrence in *rigor mortis;* when it is known to manifest itself upon arteries. Immediately after death the passive force no longer answers to its usual artificial stimulus,—the edge of the surgeon's knife; for if a limb is now amputated, neither arteries nor muscles retract, although the transient contraction of the active force may be seen to follow the course of the incision. It is an important fact, which I shall afterwards refer to, as evidencing the existence of a force in arteries for propelling the blood, that, after death, before the active muscle-force quits the body, this force may be made to respond to its own natural stimulus, namely, distension; for if an artery, at this time, is distended with water, it reacts upon its contents with a force greater than that which caused its distension.

The second fact which I adduce, in testimony that the natural motor stimulus of the passive muscle-force is supplied by nerves, exists in the evidence that is furnished by certain experiments lately performed by M. Bernard. It has been ascertained by him that when the sympathetic in the neck is divided, or its connections with the upper part of the spinal cord is cut off, or that part of the spinal cord itself destroyed, in cats and rabbits, the operation is followed by a redness and increased temperature in parts to which these nerves are distributed. These results are specially observable in the ear of the white rabbit. The correctness of the foregoing accounts has not only been confirmed by Dr A. Waller in this country; but further proof has been added by him, that the phenomena described are the direct result of the loss of the nerve-force from the bloodvessels of the part by the operation. Dr Waller has shewn that when the cut ends of the distal portions of the nerves are galvanized, the temperature and appearance of the rabbit's ear resumed their ordinary state. Professor Sharpey also exhibited the same experiments to his class during the last session; and I am authorised to state, that he concurs with the other experimenters, in the idea that these results are produced by the operation.

The redness thus produced in the ear of the rabbit appears to be caused by its substance being distended with blood; and there can be no doubt that the state of the vessels in regard to enlargement is the same as that usually described under the several phrases of "excited nutrition," "irritation," "congestion," "determination of blood," and the ordinary state of vessels in the commencement of inflammation. As regards the velocity of the flow of blood in the ear of the rabbit, I am at present unable to speak with certainty; but I would say, unless this is a special exception to the condition of bloodvessels under each of the terms referred to, which is very improbable, that the blood flows with increased rapidity. By applying to these interesting facts the theory which I advocate for the physiology of arteries, we arrive at the following definition of the laws that govern the production of these facts.

On the nerve-force being cut off from the bloodvessels in the ear of the rabbit by the operation, that slow contraction, that only contraction which is at present known to physiologists, and which is allowed by all to be under the control of the sympathetic system, is removed. This arises, not because the Tonicity, or the passive muscle-force, is removed by the operation, but because the natural motor stimulus, the nerve-force, is removed. (I have shewn it to be consistent with anatomy that motor stimulus may be generated in any nerve centre, and be conveyed by any nerve trunk, supposed to belong to either system of nerves.) The natural motor stimulus being thus withdrawn from the vessels; that large series of effects dependent on the passive muscle-force, is thrown into abeyance; and amongst these is the slow contraction which in the normal state of arteries is in permanent operation; the office of which is to adjust and maintain the size of the calibre of arteries to the ever-varying volume of the blood. By loss of the support of this contraction, all bloodvessels having muscular fibres in their walls, become dilated as the first result. By dilatation is fulfilled a condition in the vessels, whereby another law in animal physics is brought into operation, and whereby the muscular fibres are multiplied or become hypertrophied. At the same time, the blood having been admitted with greater facility into the enlarged vessels, the pulse-valve is communicated in the same ratio, and an impulse given to an augmented amount of muscular fibres at every contraction of the left ventricle of the heart. In the same increased ratio is fulfilled the condition, which is the common motor stimulus to hollow muscles,—distension, which I claim to be the natural motor stimulus of the active muscle-force in arteries, *independent of nerves*, though influenced by nerves, and which is admitted to be the case in most other hollow muscles of the body.

According to the foregoing hypothesis, an augmented active muscle-force contraction, or, in other words, increased activity of the arteries, would result in the ear of the rabbit, from the section of the Sympathetic Nerve supplying the part. I shall afterwards endeavour to prove that no distension of a tissue with blood can take place without increased arterial action, and in degree some point in a scale drawn between increased velocity of blood, and actual throbbing in the part. The exceptions to this rule are large, and occur when, from pathological states of arteries, locally or generally, the active muscle-force yields to the passive force which again takes possession of the vessels, or both forces, or rather their manifestations, may be entirely absent from arteries. By the application of this and other reasoning, I have brought into the code of the laws of animal life, that variation in the flow of blood in the capillary arteries in inflammation, which has hitherto perplexed pathologists, which exists in degrees of which retardation and stagnation may be said to be the limits of the scale.

If it should afterwards be proved that the natural motor stimulus of the passive muscle-force in arteries is not derived from nerves, and that the most that nerves contribute to that contraction is to regulate and govern, as now supposed; I should account for the absence of this contraction, and the consequent dilatation of vessels in the ear of the mutilated rabbit, by the abrupt suspension of the nervous influence. By the same rule, section of the vagi is followed by congestion in the organs to which they are distributed. In the meantime, it is fair to infer, that from the effects produced in the foregoing recited experiments, and from the fact previously adduced, that an indirect proof is offered that the natural motor stimulus of tonicity, or the passive force of arteries, is supplied by nerves.

In the foregoing part of this paper, I have brought before the medical profession the propositions, that there exist in arteries two muscle-force contractions, active and passive, the active being operative in arteries only during the diastole of the heart, whereby the blood is aided in its onward course; and that by the passive are effected all the movements in arteries at present recognised in physiology, That the active force derives its stimulus from distension of the walls of arteries; the passive force from the nervous system. I have at present supported these propositions by arguments based on the physiology of arteries. The succeeding parts of my paper will support the truth of the propositions referred to; *first*, By the application of the facts propounded to the pathology of arteries; *secondly*, By a review of the experimental evidence on which the physiology of arteries is grounded; and, *thirdly*, By a review of the arguments of the older physiologists who have contended for a blood-propelling force in arteries.

(*To be continued.*)

ART. VII.—*Thoughts, Reflections, and Remarks on Obstetric Subjects.* By JOHN BREMNER, Surgeon.

POST-PARTUM HÆMORRHAGE.

The physiology of the gravid uterus, as also the welfare and preservation of both mother and offspring, from the commencement to the termination of the impregnated state, are subjects which have deeply occupied the attention of many of the most zealous and talented of the profession in almost every age; but in none more ardently than the present, as a reference to the annals of Midwifery will abundantly testify.

That the process of parturition in the human female is of a much more painful nature, and accompanied with a far greater number of untoward and even dangerous consequences than are found to occur among the inferior orders of creation, is a fact, the reality of which must be quite apparent to the most superficial observer. This increased degree of danger depends upon causes, the principal of which I have endeavoured briefly to enumerate in two former essays on that subject, where also is contained a delineation of the mode of treatment most suitable for remedying the same.[1]

There is an affection, however, connected with the organization of the uterus in the gravid state, occurring subsequently to the birth of every member of the human family, whether male or female; and in proportion as it may exceed its natural and ordinary limits, in an equal ratio is it productive not only of disquietude, distress, and danger, but often the sudden cessation of life, to those who unhappily fall to be the victims of the same. The circumstance to which the attention of the profession is here directed, it is presumed, will be easily recognised as post-partum hæmorrhage, or discharge of blood from the uterine vessels, consequent upon the separation of the placenta therefrom; and that in every case where it is attached *in proprio situ*, and the disunion takes place agreeably to the appointed law of nature, viz., posterior to the delivery of the child.

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[1] Hints on Obstetric Practice, with Illustrations (Edinburgh, Maclachlan and Stewart, 1849); and Observations on Diseased States of the Placenta, as influencing the process of Parturition (Edinburgh Medical and Surgical Journal, No. 180.)

In considering the best means of obviating the dangers to which I here advert, it is necessary to bear in mind the following circumstances.

The connecting medium established betwixt mother and offspring is, by its structure and arrangement, intended to perform an important function in the economy of the impregnated uterus. As this function, however, is confined to the period of pregnancy, the dissolution of this connection naturally ensues at the termination of this period, and is either effected spontaneously in most instances where an exemption from disease exists, or by means of very gentle assistance, when the proper period has once fully arrived. As this assistance, nevertheless, is often required to be rendered to parturient females, it is manifest that it ought to be so efficient in its kind as, with becoming caution on the part of the patient and her attendants, wholly to liberate her from the hurtful consequences connected therewith,—the imminent danger already adverted to, into which so many parturient females, otherwise healthy, hopeful, and vigorous, are so suddenly plunged, frequently to the instantaneous deprivation of life, from the neglect of, or mal-practice in carrying into operation, the appropriate prophylactic remedies. When we reflect on these circumstances, and on the amount of distress and debility undergone throughout many future years by numbers, whose systems have been largely drained of blood, or amongst whom the discharge, though more moderate in degree, has perhaps been seldom wholly suppressed,—the subject, it is believed, will be readily admitted to be one of the most important and interesting in its nature, and loudly calling for a corresponding share of intelligence and assiduity on the part of those, who devote themselves particularly to the practice of the obstetric art.

CAUSES.—It is unnecessary to devote much time to the consideration of the causes of uterine hæmorrhage, seeing it is invariably produced in consequence of a non-contracted state of its bloodvessels after the separation, and also very frequently the expulsion of the placenta.

In taking a retrospective glance at the remedies which have from time to time been introduced to the notice of the profession, through the medium of the different medical periodicals, for the arrest and removal of this most formidable and appalling affection, they will be found to be both numerous and diversified in their nature. In addition to the ancient and invaluable application of cold in its varied forms, and the use of astringents generally as remedial agents, there have, during recent years, been proposed and successfully carried into practice, the use of galvanism by Drs Simpson, Radford, and Dorrington,—opium by Dr Griffin,—ergotine by Drs Ebers and Beattie,—the bowl or basin of Mr Harvey,—the bandage and pads of Mr Pretty,—transfusion and the injection of stimulants by Dr Torbock and others,—the caoutchouc tube of M. Diday, besides a host of other equipments.

In making mention of the preceding list, it is neither my intention to offer any remarks regarding the relative merits of propositions so strongly advocated by their respective authors, and to any of which I have, during a lengthened series of years, on a very few occasions only, been compelled to have recourse, or to swell their number by the introduction of any fancied discoveries of my own, to which a greater degree of confidence could be warrantably attached; but simply, by means of a brief anatomical description of the uterus, both in its natural and gravid state, to endeavour to ascertain whether or not the sanction of measures could possibly be obtained, which, by acting as a *caveat* against the occurrence of hæmorrhage, would thereby obviate the necessity for calling in the aid of the majority of these or any other applications.

"The uterus," says M. Cloquet, "is composed of an external or serous membrane, an internal or mucous membrane, an intermediate proper tissue, nerves, and vessels." It is in this intermediate tissue that all the expansive and contractile power resides, connected with the enlargement and expulsion of the fœtus, and finally in its reduction again to its original dimensions. The description of it given by the same author, is as follows:—"It occupies the interval which separates the peritoneum from the mucous membrane. Its thickness is great, and amounts to five or six lines. It is of a dense and close texture, and yields great resistance to the knife. It is elastic, and of a grayish-white colour. Its adhesion to the mucous membrane is very great. Its intimate nature is yet little known. It is traversed by a very great number of bloodvessels. It is impossible in any part of its extent to ascertain the description of the fibres of which it is composed. Towards the neck it is denser and less gray than in the walls of the body. It would appear that during gestation this tissue becomes truly muscular."

So much for the history of the uterus in its unimpregnated state. When, however, conception has once become established, it gradually augments in volume, till, from having been a small closely-contracted organ of two inches in length, it attains a magnitude capable of containing not only one, but often two, and occasionally three, and even four or five fully organized fœtuses, together with their secundines, besides, in many cases, a large collection of liquor amnii.

The bloodvessels which supply the uterus likewise become greatly enlarged, and are thus described by M. Cloquet (p. 817):—"The arteries of the uterus come from the spermatic and hypogastric; their principal branches are placed beneath the peritoneum. They are very flexuous, and anastomose frequently together. The veins follow the same course, but are still more flexuous, and form cavities in its walls which become very large during gestation, and which are called *uterine sinuses*." And again, at page 712 he observes of the uterine artery:—"Its volume is always in relation with the state of development of the uterus, so as frequently to be very small; whilst in other cases,—at the end of gestation, for example,—it is larger than any other branch of the hypogastric artery," &c.

In reviewing the changes which invariably take place in the organization of the uterus, posterior to the period of conception, it will be readily perceived, that expansion of volume and increase of the sanguineous current throughout the structure of the organ, form the chief. It demands to be observed, however, that, although the development of the fœtus renders a more abundant supply of blood essentially necessary during the period of gestation than at other times, the experience of many years has served fully to convince me, that in numberless instances the quantity is considerably greater than is absolutely requisite for the purpose in question. The effect of this is, that presently a plethoric state of the system is induced,—denoted by vascular excitement, &c.,[1]—and which, if not timely counteracted, affects, with few exceptions, some of the particular organs or distinct portions of the body,—most frequently the uterus itself, towards its orifice, and parts adjacent,—thus constituting what is usually, though erroneously, termed "rigid uterus," and operating as one of the chief causes in the production of tedious or laborious labour.[2]

The intimation requires scarcely to be given, that the dorsal and upper extremity of the organ is the situation set apart by the Creator as the most suitable for the attachment of the placenta;—the bloodvessels, though enlarged, existing only in little more than miniature in respect of where they effect their ingress near the cervix. This arrangement forms a wise and safe provision on behalf of the parturient female, seeing that the disposition to hæmorrhage, notwithstanding its frequency, is reduced to the smallest ratio possible, in comparison with what is witnessed in those melancholy instances where it is implanted in the opposite direction. It may likewise be observed, that, united in the *natural form*, as it may with propriety be termed, the placenta occasions no interruption to the progress of labour, whilst at the same time its vicinity to the fundus, the portion of the organ where contraction after delivery commences with only the most solitary exceptions, affords us every desirable facility for averting the occurrence of hæmorrhage in almost every case in which we are entrusted with the superintendence of the labour.

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[1] See Hints, &c., formerly referred to, pp. 8–11.

[2] *Op. cit.* p. 12, *et seq.*

Before proceeding farther in the investigation, a question of very considerable importance presents itself for consideration. Seeing that for beneficial purposes connected with gestation, the uterus is uniformly furnished with an extra supply of blood,—frequently to the surcharge of the general system,—and likewise that upon the separation of the placenta from its parietes, hæmorrhage does and must to a certain extent at least, ensue,—how far can the discharge of blood in this manner be permitted, consistently with the safety and welfare of the parturient female,—and is the same law applicable under all the variety of cases and constitutions that are daily met with in practice? Or, in other words and a more restricted form,—By what general law can discharges of blood from the uterus posterior to delivery be regulated, which shall not only be exempt from danger, but prove in every way conducive to the comfort and convalescence of all classes and descriptions of in-lying females in every region of the globe?

In tracing the history of the MAMMALIA and other quadruped animals, both in a wild and domesticated state, we obtain satisfactory evidence, that they enjoy well-nigh a total exemption from hæmorrhage connected with the birth of their young; or, should any evacuation of blood occasionally take place, it is only in the relation of shadow to substance, to what is in everyday practice witnessed amongst the noblest of all the feminine creation, "woman,"—being rarely productive of the smallest injury or danger, where exemption from disease exists, excepting in cases where injudicious interference has been employed.[1]

Having ascertained a good many years ago the reality of what has presently been stated, viz., the comparative absence of hæmorrhage amongst the lower animals, I was led, not only by inference and reflection on the subject, but also by means of observations resulting from a few obstetric cases, to imagine that a similar issue, in so far as it could be conditionally carried into operation, would be attended with an equally salutary effect in the human female.

In order the better to develop the nature and importance of the proposition under consideration, and at the same time to test more thoroughly the validity of the measures to be adopted for its accomplishment, it is found necessary to adduce a few additional statements, chiefly in the way of query, arising partly out of those lately exhibited.

1*st*, That the uterus being in every case supplied with a redundance of blood, and seeing that the separation of the placenta therefrom presents a favourable opportunity for its transit from the system,—is it or not in accordance with the dictates of experience and physiological research, that its removal in this manner has been ascertained to be productive of benefit or injury.

2*d*, That in the event of this being found unprofitable or injurious in ordinary cases, especially where considerable delicacy of constitution and weakness prevail,—would it not be considered a plan in every way eligible amongst all those in the enjoyment of a more robust state of health and strength; and likewise in every instance where symptoms denote the general system to be in an excited and surcharged condition, —and thereby prove instrumental in averting the occurrence of peritonitis or similar inflammatory attacks, with which puerperal females are, from this cause, so extremely liable to be affected ?

3*d*, That as there is observed to exist in a good many females a peculiar proneness to hæmorrhage immediately upon the birth of the child, frequently in such a current and degree of impetuosity as occasionally to prove suddenly fatal to life, and always requiring the most energetic measures for its suppression,—in the event of its reduction to the lowest ratio which means are capable of accomplishing, can the desired success be calculated upon in the description of cases here specified also, or must they constantly remain exceptions to the general rule?

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[1] To the consideration of this topic it is my intention again to revert at a future period of the inquiry, for the purpose of investigating the causes which seem chiefly to give rise to the diversity above mentioned.

4th, That as *post-partum* hæmorrhage depends on a non-contracted state of the uterine vessels,—what are the measures which have received the sanction of experience as most deserving of confidence in effecting this object, as well as the most suitable period for, and manner of, their application?

(*To be continued.*)

ART. VIII.—*Statistical Report upon Cases of Disease of the Heart occurring in St George's Hospital, London; especially in relation to Rheumatism and Albuminuria.* By Dr BARCLAY, the Medical Registrar. (From the Medico-Chirurgical Transactions, London, Volume the Thirty-Fifth. Read November 11, 1851. London, 1852.)

I. EXAMINATION OF FATAL CASES IN THEIR RELATION TO RHEUMATISM AND ALBUMINURIA.

Since drawing up the Report on Valvular Diseases of the Heart, which the Society did me the honour to publish in the 31st Volume of its Transactions, my attention has been a good deal turned to the same subject; and it did seem that some of the questions connected with the pathology of this organ might be elucidated by an accumulation of carefully recorded clinical observations, not in a few selected instances, but throughout the whole range of cases supplied by a large hospital; and thus the appointment I hold be made subservient to the advancement of science. It is true that the most certain results are those derived from *post-mortem* examination; yet there are circumstances connected with structural change which can only be learned at the bed-side of the patient. We have no means of learning the history of rheumatism after death; and the permanent presence of albumen in the urine during life is of greater importance than structural alterations in the kidney revealed by dissection. It therefore became a matter of importance that the table of *post-mortem* appearances should only include those whose clinical history was known as having a bearing on these subjects; and hence, patients dying in the medical wards only are considered, and from among these all such are excluded as were brought to the hospital dead or dying, all whose history contains no allusion to disease of the heart during life, and all in which partial evidence of old pericarditis, or specks of atheroma on the valves were found as the only condition of disease after death, which could give rise to no symptoms, and therefore could have no history.

The present communication is not confined to valvular lesion, because pericardial is so bound up with endocardial disease, and both together with alterations in the muscular structure, that it becomes impossible to study one without considering the other morbid changes to which the heart is more or less liable. Some of the deductions differ in a few particulars from those which seemed warranted by the previous collection of cases; and while it is true that my opportunities for obtaining accurate information have been greater, it is also true that the liability to error always increases in an inverse ratio to the number of instances. This circumstance only lays on me the greater obligation to present to the Society this additional Report.

The cases are arranged in the Table in the following order:—1*st,* Cases of acute rheumatism fatal during the rheumatic attack; 2*d,* Cases of recent pericarditis, independent of recent rheumatism; 3*d,* Cases of recent lymph on the valves not previously recorded; 4*th,* Adherent pericardium and extensive old pericarditis, with no recent condition of inflammation; 5*th,* Remaining cases of old valvular disease divided into thickening and atheroma; and, *lastly,* Cases in which alteration in muscular structure was the prominent feature. Under each head rheumatic cases generally stand first, and the rest either in distinct groups or following the order of ages.

1. *Cases of Acute Rheumatism fatal during the Rheumatic Attack.*

Of this there are eight instances. In two of these, suppuration had occurred within and around the joints, and it has been questioned whether such are not of a nature somewhat different from ordinary rheumatism; in one of them, however, the pericardial membrane was in the first stage of inflammation, and in the other, both aortic and mitral valves were opaque, a condition which would seem to be allied to inflammation. Five cases are examples of rheumatic pericarditis in its ordinary form, one of which derives interest from the circumstance, that no friction sound was ever heard during the continuance of the patient in the hospital, although from the alteration of rhythm it was often listened for, and the pericardium was found filled with recent lymph, and its two layers only very partially adherent to each other. In three of them there was also evidence of previous inflammation of the pericardium.

In three of these cases of pericarditis, distinct evidence was also found of recent endocarditis, associated in two instances with old disease. In the only instance in which recent lymph was found on the valves after a first attack of acute rheumatism, it was confined to the mitral valve. Although it appears that pericarditis existed in a considerably larger proportion of these cases than endocarditis, it does not follow that it is a more common, but only a more immediately dangerous complication of acute rheumatism, because in each instance the obstruction to the circulation consequent upon it, was the chief cause of the fatal termination.

The remaining case is one of great interest; because during life an endocardial murmur at the base of the heart was distinctly made out at an early period of the disease, and yet no valvular lesion existed. The history is shortly this. S. A—, æt. 23, unmarried, catamenia regular, of pretty full habit of body, and good complexion, was seized with pains in her joints on the 17th January 1850; had been under treatment since the 19th; and was admitted into the hospital on the 24th, with a first attack of acute rheumatism. She complained of some catching in the breathing; and a slight roughness with the first sound of the heart was first heard on the 26th, and a distinct systolic endocardial murmur was heard at the base of the heart on the 28th. She became slightly delirious, with tremulous movements, on the 1st February; the delirium was very violent on the evening of the 2d; and she died comatose on the morning of the 3d. Throughout the case there was great febrile disturbance; the face was flushed, and the pulse full and frequent; and after death large clots of recent lymph were found in the synovial cavities.

With such a train of symptoms, it would not have been unreasonable to suppose that a gradually increasing endocardial murmur, heard first on the tenth day of her illness, depended on the presence of vegetation on the aortic valves; and hence its importance, as shewing that in the acute stage of rheumatism, with no indications of impoverishment from other causes, the physical properties of the blood may be so altered, as to produce a *bruit de soufflet,* without any alteration of structure, either within or without the heart.

2. *Recent Pericarditis independent of recent Rheumatism.*

The Table contains sixteen[1] examples of this condition, four of which are associated with traces of old disease. Nos. 9 and 10 seem directly due to inflammation of the peritoneum spreading through the diaphragm,—the one alleged to have been caused by injury; the other known to have supervened on paracentesis abdominis. No. 11 probably owed its origin to extensive and severe pleurisy, with which, at the least, it was associated. In none of these did rheumatism precede or accompany the attack. In No. 10 there was also albuminuria; and there are besides six cases, Nos. 12–17, in which pericarditis occurred just before death in persons of a cachetic aspect, from long-continued disease of the kidney. It was accompanied in one case by recent pleurisy, and in another by diffuse cellular inflammation; these had never had rheumatism; in one no record has been kept; while in another there had been a rheumatic attack eight months before admission, and in this case only was the pericarditis of very severe character. In the remaining case, No. 13, the patient had suffered from acute rheumatism twenty-six years before, with probably corresponding disease of the mitral valve; but there had been no recent attack.

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[1] Among the cases omitted in consequence of their having no clinical history, is one of a patient admitted dying of dysentery, in which the only discoverable lesions were extensive ulceration of the large intestine, and pericarditis.

In No. 18, none of the preceding causes was in operation; the patient had never suffered from rheumatism, the urine was not albuminous, and there was nowhere else inflammation of the serous membranes; but there was lymph on the endocardium and valves, with congenital malformation; and, as the most prominent feature of the case during life, tumultuous action of the heart. It was pointed out in the former report, that malformation of the valves almost always leads to further changes, an observation I am glad to find confirmed by Dr Ormerod;[1] and in these cases it would appear, that the exciting cause is the increased action induced by an impeded, and especially regurgitant circulation. The same principle probably applies to the pericardium; and in closely watching those fatal cases of albuminuria which have been associated towards their close with pericarditis, it has seemed to me that the turbulent action of the heart has played a most important part in developing the disease. Indeed, it must be evident, that albuminuria acts more especially in establishing a predisposition to serous inflammations; and in this, differs entirely from rheumatism, which, for the most part, acts directly as an exciting cause of cardiac inflammation, and within certain limits, in proportion to its severity. In No. 16 it is probable that the albuminuria had less to do with the pericarditis than the general disturbance to the circulation, caused by the altered relations of the chest to the heart and great vessels, in consequence of spinal curvature, which gave rise to a loud systolic murmur, for which no other cause was found after death, as the heart was free from valvular lesion. Another case also deserves notice, from the circumstance that the condition of albuminuria with which the pericarditis was associated, appeared to depend on skirrhous obstruction to the ureters, giving rise to dilatation and dwindling of the kidneys.

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[1] Gulstonian Lectures at the College of Physicians, *anno* 1851.

In the four cases in which old and recent inflammation are found associated, no especial cause for its occurrence demands notice; but if it were true that old inflammation acted as a predisposing cause of a fresh attack, it ought, perhaps, to have been found in a larger number of the cases given in *this* Table.

Four cases of pericarditis next claim our attention, in which the deposit of lymph was not quite recent. In Nos. 24 and 26, there is a history of rheumatic fever occurring, in the one twenty-five, and in the other six years before, with no subsequent recurrence; they were free from symptoms referable to the heart, until after what was described as a "severe cold" with pain in the chest, at four and six months before admission; both were unhealthy subjects, and one had disease of the kidney. In No. 25 there would appear to be nothing to fix the date. In No. 27 there is also chronic thickening of the membrane, indicating a previous attack, both apparently corresponding to *post-mortem* appearances of two successive attacks of peritonitis, during the latter of which the patient was under observation; but the existence of pericarditis, if present at that time, was not observed.

3. *Recent Endocarditis independent of recent Rheumatism.*

There are four instances in which, without any evidence of previous inflammatory action, recent lymph was found on the valves; in Nos. 18 and 28, associated with malformation of the aortic valves; in Nos. 24, 28, and 29, with disease of the kidney; and in No. 24, with a report of acute rheumatism six years before, of which, however, no trace had been left in any alteration of structure about the heart.

There are seven cases in which recent lymph was found associated with previously existing disease of the valves; in No. 17 alone, is there no history of rheumatism; in Nos. 21 and 32 there was calcareous deposit; and it may be questioned whether such a condition can be traced to acute rheumatism as its source, but in the others, the probability is fair, that the primary disease was rheumatic endocarditis; and it is remarkable how much more frequently the thickening, arising out of this condition, is associated with recent inflammatory action than any other form of old valvular lesion. It was associated, in one case, with an attack of pleurisy; in another, with acute bronchitis; two had albuminous urine, but did not exhibit an advanced state of disease of the kidney.

In two cases lymph was found, "not quite recent," on the valves, Nos. 35, 36, probably corresponding to rheumatic attacks, which were reported to have occurred some months before death. In both of these the deposit was found only on the mitral valve; as was also the case in the only instance, No. 4, in the table, of rheumatic endocarditis without previous thickening. Of the two cases in which malformation existed, No. 18 had recent lymph on both sets of valves, and No. 28 only on the aortic. In the two cases combined with albuminuria, recent lymph was found in one on the mitral, and in the other on the aortic valves. Among the cases in which recent lymph was found associated with old disease of rheumatic origin, it occurs in three instances on both sets of valves; in three on the aortic alone; in two on the mitral alone. The only case in which old thickening and recent lymph were found together, without rheumatism, No. 17, had both conditions limited to the mitral valve.

4. *Old Pericarditis.*

In a large proportion of the *post-mortem* examinations, traces of old inflammation of the pericardium are recorded; in some the membrane being universally adherent, while in others the adhesions were more or less limited, or patches of partially absorbed fibrin were observed on its surface. In very many instances clinical history fails to throw any light on the origin of these attacks, and we can do little beyond inquiring in what proportion there is a history of previous rheumatism, or evidence of albuminuria; although it must be remembered that it does not follow, even when present, that either was the efficient cause of the attack.

Traces of previous inflammation are found in eleven out of twelve cases reported to have suffered from acute rheumatism; and in twelve out of thirty-one reported to have suffered from any kind of rheumatism whatever; but the single instance (69) associated with slight rheumatism is of very trivial character. In all the cases in which it is associated with a distinct history of acute rheumatism, the pericarditis appears to have been of considerable extent. There are fifteen other instances of severe old pericarditis, thirteen of which are reported never to have suffered from rheumatism.

There are six cases in which the pericardium was universally firmly adherent; one with a history of acute rheumatism five years before; one in which there is no history of rheumatism, with malformation of the aorta and old pleurisy; and four who never had rheumatism, in three of which the kidney was diseased, two had old pleurisy, and one old peritonitis.

In three cases, the adhesions were nearly universal, with histories of acute rheumatism, one having also had pleurisy. In seventeen cases the pericarditis was considerable, but there were either no adhesions, or they were very slight: six of these correspond to histories of acute rheumatism; one had rheumatic fever only six months before, with very old pericarditis, and also aneurism of the aorta; in one there was old pleurisy, with no history regarding rheumatism; and nine are said never to have suffered from it. Of the last, it was associated in four instances with disease of the kidney, in four with more or less severe pleurisy, one with peritonitis, one with aneurismal dilatation of the heart, and in one no distinct cause was observed.

It is worthy of remark, that while all the cases of rheumatic pericarditis appear to have been severe, only one has distinctly terminated in complete adhesion; and on the contrary, the majority of the cases of adherent pericardium occur among persons who have never suffered from rheumatism at all. This series of cases does not seem to indicate that complete adhesion is the most favourable termination of the attack, whether the ages of the patients or the condition of the heart be taken as an indication.

5. *Valvular Lesion.*

If it be true that very many cases of pericarditis take their rise independently of acute rheumatism, the same fact is only more plainly indicated in regard to valvular lesion. After the admirable lectures on this part of the subject delivered by Dr Ormerod in the present year, I shall do little more than point out wherein the present series of cases differs from that which I formerly collected.

Including recent cases with those of longer standing, there are in the table sixty-two cases of valvular lesion; of which—

23 are associated with a history of acute rheumatism, but in three of these the lesion did not appear to be of rheumatic origin.
26 occur in persons who never had suffered from rheumatism at all.
8 are cases (chiefly of slight disease) in which no record on this subject has been kept.
2 are associated only with slight rheumatism, in which the lesion does not appear to have had an inflammatory origin.
1 was said to have been "slight rheumatism," and
1 said to have been "gout;" in each of which the probability is, that the disease was primarily rheumatic endocarditis.
1 was hereditary gout, with a deposit of lithate of soda on the valves.

There are therefore twenty cases, in which we may be tolerably certain that the disease was rheumatic, and twenty-six in which it was positively denied that the individual had ever suffered from rheumatism at all. From these two classes we obtain the following numbers as representative of the liability to disease of the aortic and mitral valves:—

	Rheumatic.	Non-Rheumatic.
Disease affecting both sets of valves	12	10
Disease of mitral valve alone	7	11
Disease of aortic valve alone	1	5

Whence it appears, that there is a greater liability in rheumatic cases to simultaneous lesion of both sets of valves, as well as a greater preponderance of disease of the mitral over the aortic valves in them, than in non-rheumatic cases.

This may be further illustrated by examining more clearly the cases in which both valves are implicated. It appears that one valve is described as being in a more advanced state of disease than the other, in five of the twelve rheumatic cases, and only twice among the ten non-rheumatic. In the former it is always the mitral that is most affected; in the latter it occurs once in each set of valves. Again we have found in the recent cases, that when not associated with previous thickening, the lymph was always found on the mitral valve in rheumatic cases, while in non-rheumatic or doubtful cases it was twice found on the aortic valves, and once on the mitral. In the majority of instances, therefore, in this table, rheumatic endocarditis was first marked by a deposit of lymph on the mitral valves, followed by thickening and contraction; and there, with or without a renewed attack of acute rheumatism, inflammation was again readily set up, spreading in most instances also to the aortic valves.

Of nineteen instances in which a history of rheumatic fever some years previously was distinctly made out, fifteen indicate severe valvular lesion of long standing. In three of these, atheromatous or calcareous deposit was found along with old thickening; in which it must always be to a certain extent doubtful, whether the atheromatous disease have arisen out of the purely inflammatory alteration in texture, produced by acute rheumatism, or out of those independent conditions, from which it more generally originates. In the one view, the thickening would be the primary lesion; in the other, it would only be secondary. And this series appears, like my former collection of cases, to contain examples of both conditions. In almost all the cases in which recent lymph is found in addition to old standing disease, a history of having at one time or other suffered from acute rheumatism is obtained.

With regard to the association of albuminuria and valvular lesion, it may be stated generally, that, taking into consideration all conditions of disease of the valves, there were found,—

In 22 instances, diseased kidney, with albuminous urine;
In 26 instances, healthy kidney; and
In 13 instances, evidences of disease, which were more or less obscure.

The tricuspid valves are stated to have been diseased in ten instances. In six they were thickened; three being associated with a history of acute rheumatism, and two with Bright's disease. In two instances these valves were affected with atheroma; in one they were opaque; and in one atrophied. In the last instance the pulmonic valves were also affected with atrophy. No other instance of disease of these last occurs in the Table.

6. *Hypertrophy and Dilatation.*

Scarcely a single instance occurs in the Table of old lesion of the valves or pericardium, in which alterations are not found in the thickness of the walls, or the dimensions of the cavities of the heart. But these are not the only causes of change; atheromatous degeneration of the arterial coats exerts considerable influence; and the rationale of this is simple enough. It implies the loss of one of those conditions of health, which are the most favourable to the onward flow of the blood; and hence arises either accumulation of blood in the heart, in the form of dilitation, or, what is much more common, a compensating power in increased thickness of its walls, to overcome the retardation. Albuminuria also acts as a cause of alteration, chiefly in the direction of hypertrophy; but it is one for which it is very difficult to find a reasonable explanation. It is not that disease of the kidney first makes itself felt in valvular lesion, or inflammation of the pericardium, and only secondarily in alteration of dimension; but the two conditions are so often found coincident, when clinical investigation and post-mortem examination alike fail to detect any other cause for the disturbance of the circulation, that we cannot withhold our assent to the proposition, that they bear some relation to each other. Some idea may be formed of the frequency of this association, from the circumstance that in going over the post-mortem records at this hospital for a period of about three years, I find 141 cases in which the kidney is noted evidently diseased; and among these hypertrophy is noted as occurring 55 times, and dilatation 36 times, either together or separately, in sixty-three individuals, while there are only seventy-eight instances, very little more than half, in which the heart has retained its natural dimensions.

These alterations are less easily recognised during life, then when complicated with valvular lesion; and consequently these two causes (although always stated when present in cases finding place in this Table), may not be enumerated so frequently as they actually occur in proportion to other lesions having a similar tendency. Nevertheless an attempt may be made by placing them in a tabular form to shew their several relations to conditions of greater or less hypertrophy and greater or less dilatation.[1]

	Simple hypertrophy.	Considerable hypertrophy, comparatively little dilatation.	Great hypertrophy, with great dilatation.	Moderate hypertrophy, with moderate dilatation.	Considerable dilatation, with little hypertrophy.	Great dilatation, with more or less hypertrophy.	Simple dilatation.
Number of Cases.	15	6	3	17	7	10	12
Adhesions of pericardium, . .	1	3 or 5 ?	2 or 3 ?	3	2 or 3 ?	2	4 or 6 ?
Disease of aortic valves,	4	2	1 or 2 ?	3 or 4 ?	3 or 4 ?	4 or 5 ?	2 or 4 ?
Disease of mitral valves,	3 or 4 ?	3	1 ?	8 or 9 ?	2 or 3 ?	6	5 or 6 ?
Atheroma of aorta,	9	3	1	6	2	1	1
Disease of kidney,	9 or 11 ?	3	2	11	2 or 3 ?	3	3 or 6 ?
None of the above causes, . .	1	1	0	0	0	1	2

It would appear from this arrangement of the cases, that diseases of the aortic valves is rather more associated with hypertrophy than dilatation, the latter occurring chiefly with regurgitation, while disease of the mitral valve is more decidedly associated with dilatation. Adhesions of the pericardium are also more associated with dilatation than with hypertrophy. Atheroma of the aorta and disease of the kidney each accompany a large majority of the cases of simple hypertrophy, and the former in particular seems to have very little to do with cases of dilatation. Of course it is impossible to assign to each, among so many causes of change, its own particular value, except in a very general manner, when the two states are conjoined.

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[1] It is plainly necessary to exclude such lesions of either set of valves as cannot be any impediment to the onward flow of blood, or cause of regurgitation, and also cases of pericarditis, which merely consisted in a patch of old lymph on the surface of the heart. When the amount of disease might or might not have deranged the circulation, the cases are marked as doubtful and where albuminuria did not exist during life, altered structure of the kidney discovered after death, are also enumerated as doubtful causes of alteration. The last of two numbers placed together includes these doubtful cases.

One case of aneurismal dilatation, one of aneurism of the aorta, and one of congenital constriction, are excluded.

II. GENERAL STATISTICAL RELATIONS OF RHEUMATISM TO DISEASES OF THE HEART.

The whole number of patients admitted into the medical wards of any large general hospital would very soon supply an accurate basis for statistical relations between two diseases of such common occurrence as rheumatism and disease of the heart, were the facts but sufficiently simple, or capable of being resolved into elements on which reliance could be placed. But we are unfortunately, as yet, in ignorance of the essential element of rheumatism, and the most practised ear may be deceived in a diagnosis of disease of the heart. In entering on such a field it therefore becomes necessary to select such definitions as may, at least, be intelligible, and by acknowledging the sources of fallacy, and endeavouring to exclude the cases affected by them, to obtain credit for accuracy in the remainder. For the purposes of the present inquiry, rheumatism has been simply divided into acute, subacute, and chronic, because the first is that especially in which cardiac inflammation is liable to occur.

I have adopted for acute rheumatism, the somewhat antiquated definition of inflammation, "rubor et tumor, cum calore et dolore." If all these be present, gout being excluded, the case is classed as one of the acute form; if redness be absent, unless there be very marked febrile disturbance, or rapid transition from one joint to another, mere puffiness and tenderness are only taken as evidence of a subacute form,[1] to which is also consigned the whole class of what has been called synovial rheumatism. This definition has the advantage of making the question intelligible to the patients themselves whether any rheumatic attack they may have experienced has been acute or not. It is, indeed, difficult to say what relation chronic rheumatism bears to what has been well denominated rheumatic fever; and of subacute rheumatism it may probably be said with truth, that some instances are merely a milder form of the same disease, while others have very little in common with it. In children especially, it is found that great febrile disturbance is often present, indicating true rheumatic fever, with scarcely any external signs of inflammation.

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[1] I believe I have erred on the side of too great exclusiveness, because, until very recently, I had never seen inflammation of the heart *commence* in the subacute form; and no such case occurs among the reports on which this paper is based. Hospital practice is not the most favourable for ascertaining such a point, and the advantage of increased accuracy was not lightly to be thrown aside, when the facts seemed to warrant the conclusion that inflammatory action was restricted to the acute form. The doubtful cases are few. One, a lad of 17,

The case already detailed (page 91) shews what caution must be exercised in forming a diagnosis of endocarditis commencing in an attack of rheumatism. The stethoscope will not serve alone for a diagnosis; and, therefore, where other signs of endocarditis are wanting, the case must be regarded as doubtful. With pericarditis, the difficulties, though not less, are of a different character; here sometimes the sound is so transient that it is not caught until the roughened surfaces have come into opposition, and one instance has been mentioned (page 90), in which all the requisites being found for its production, except perhaps the presence of fluid, of which there was none, no friction was ever heard. Neither can the ear take cognisance of previous attacks. But once heard and recognised I am not aware of any fallacy that attends the existence of friction as an indication of pericarditis. Here the error is on the side of omission; in endocarditis the error is on the side of excess.

suffering also from phthisis, who had been ill only a week, but had been obliged to keep his bed the whole time, admitted with a first attack of rheumatism, not presenting any symptom of an acute form, save a quick and excited action of the heart, with a *bruit de soufflet* heard, first over the aortic valves, and two or three days later more loudly towards the apex, and persistent. Another, a lad of 15, who had been suffering from rheumatism for six weeks before admission, and had palpitation of the heart, according to his own account, some weeks previously, having only experienced a very slight attack of rheumatism four years before, was believed to have disease of the aortic valves. In addition to these, there were two cases of a first attack of rheumatism, of eight and ten weeks' duration respectively, which so far as could be ascertained, had probably commenced as acute rheumatism, and had degenerated into the subacute or almost chronic form, in which they were admitted, with cardiac affection. There are also six examples in females suffering from subacute rheumatism, of a *bruit de soufflet* being present with no history of an acute attack; but in these the abnormal sound was believed to be merely functional. In the table of *post-mortem* examinations only one case occurs, where, with a distinct history of rheumatism never of an acute character. the lesions are such as might have been due to rheumatic inflammation of the heart at an earlier period. Quite recently I have met with a case in which pericarditis commenced in an attack altogether unaccompanied by acute symptoms, not included in these statistics

1. *Of Rheumatism generally.*

The cases, of which notes more or less extended have been kept, include 152 of acute rheumatism, 178 subacute, and 358 chronic. Exclusive of doubtful cases, these give severally the following proportions of cardiac complications:

	Total.	With Cardiac Complications.	Per cent.
Cases of acute rheumatism	152	67	44·0
„ subacute „	178	20	11·2
„ chronic „	358	14	3·9

In all the chronic cases,[1] and in the majority of the subacute, in which the heart was affected, there was a history of previous rheumatism of a more severe character: and in regard to the former, there can be no doubt that the disease was of a date prior to their admission.

The two sections of acute and subacute cases may be arranged in periods of years, indicating the proportion for different ages of first and subsequent attacks, and also the number suffering from cardiac complications; but it must be remembered that these are not the ages at which disease of the heart begins.

ACUTE ON ADMISSION.

Age on Admission.	No previous Rheumatism.	Have had Rheumatism.	Total.	With Cardiac Complications.	Per cent.
Under 10 years	2	0	2	1	50·0
Over 10 under 15	3	3	6	6	100 0
„ 15 „ 20	24	14	38	20	52 6
„ 20 „ 25	27	15	42	19	45·2
„ 25 „ 30	14	11	25	7	28·0
From 30 to 40	13	16	29	11	37·9
Over 40 years	3	7	10	4	40 0

SUBACUTE ON ADMISSION.

Age on Admission.	No previous Rheumatism.	Have had Rheumatism.	Total.	With Cardiac Complications.	Per cent.
Under 10 years	2	0	2	0	0·0
Over 10 under 15	3	4	7	4	57·1
„ 15 „ 20	21	18	39	7	17·9
„ 20 „ 25	23	10	33	4	12·1
„ 25 „ 30	22	8	30	2	6 6
From 30 to 40	16	13	29	2	6·9
Over 40 years	20	18	83	1	2·6

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[1] Two cases of chronic rheumatism are omitted, in which, never having had rheumatism previously, old disease of the heart with hypertrophy was found on admission, which could not have had a rheumatic origin.

The chief point of interest here is to observe that the two classes bear pretty nearly the same proportion to different ages up to 25; and subsequently the numbers suffering for the first time from rheumatism are very much more frequently found in the subacute than in the acute class. It is also worthy of note, that while cardiac complications generally bear a much larger proportion to the acute than to the subacute cases, the ratio is less dissimilar before 25 years of age than it is after it. These two observations serve to illustrate each other.

2. *Of Acute Rheumatism.*

As a most unquestionable cause of inflammation of the heart and pericardium, it becomes of interest to inquire not only in what proportion of instances, endo- and pericarditis are associated with acute rheumatism, but also if possible to ascertain what influences external circumstances of age and sex have in determining the cardiac affection. For this purpose it is essential to distinguish first and subsequent attacks of rheumatic fever, because when an endocardial murmur is heard, it is not always possible to determine whether it depend upon recent or on old disease, and knowledge of a previous attack admits the possibility at least of its having a prior existence. The proportions will be best seen in a tabular arrangement:

Cases of a first attack of acute rheumatism			88
Males			38
Cases of endo- and pericarditis	14	36·9 per cent.	
Doubtful cases	4	10·5 „	
Cases with no cardiac affection	20	52·6 „	
Females			50
Cases of endo- and pericarditis	22	44·0 per cent.	
Doubtful cases	9	18·0 „	
Cases with no cardinal affection	19	38·0 „	
Cases of a subsequent attack of acute rheumatism			64
Males			38
Cases of endo- and pericarditis, or of old disease	17	44·7 per cent.	
Doubtful cases	5	13·2 „	
Cases with no cardiac affection	16	42·1 „	
Females			26
Cases of endo- and pericarditis or of old disease	14	53·9 per cent.	
Doubtful cases,	4	11·5 „	
Cases with no cardiac affection	9	34·6 „	

All cases together:	152	
Cases of endo- and pericarditis, or of old disease	67	44·1 per cent.
Doubtful cases	21	18·8 „
Cases with no cardiac affection	64	42·1 „

From this table it will be seen that among this collection of cases there is a greater proportion of females than of males admitted with a subsequent attack, and, *vice versâ*, a larger proportion of males with a subsequent attack; whence we may conclude, if these proportions hold generally true, that on the whole females are somewhat more liable to the disease than males, but that the latter are more exposed to its recurrence. Similarly, it appears that females are in a small proportion more liable to cardiac complication than males; that this excess is not due to functional murmurs being mistaken for endocarditis is proved by the fact, that in twenty-eight of the sixty-seven cases of heart affection, pericardial friction was noted, and of these eighteen were females, and only ten males. As might have been anticipated, the proportion of cases with old or recent disease of the heart is increased in the subsequent attacks, about 18 or 20 per cent., so that of patients who have escaped in their first seizure, one in five may be expected to suffer from it in any subsequent attack.

The influence of age is considerable, but not so great as mere post-mortem evidence led me formerly to conclude. Excluding the doubtful cases, we derive the following proportions for different ages of heart affections attending on a first attack of acute rheumatism.

Age on Admission.	Total.	Heart Affected.	Per cent.
Under 10 years	2	1	50·0
Over 10 under 15	4	4	100·0
„ 15 „ 20	18	13	72·0
„ 20 „ 25	23	10	43·5
„ 25 „ 30	14	4	28·5
From 30 to 40	13	4	30·7
Over 40 years	1	0	0·0

In the greater number of those suffering from subsequent attacks, the date of their first attack is preserved.

Age at First Attack.	Total.	Heart Affected.	Per cent.
Under 10 years	5	4	80·0
Over 10 under 15	14	8	57·1
„ 15 „ 20	9	6	66·6

Over 20 „ 25	12	6	50·0
„ 25 „ 30	6	3	50·0
From 30 to 40	6	2	33·3
Over 40 years	1	1	100·0?

It is not meant to be alleged that in all these instances the first attack was that in which the disease of the heart commenced; on the contrary it is to be remembered that not only are there among these cases in which inflammation of the heart commenced along with their subsequent attack, but also cases must be included which were not due to rheumatic inflammation at all, and in which the diseases were merely coincident: the last case in particular was believed to be one of this sort, where the disease of the heart was believed to have been prior to his first rheumatic attack.

The age on admission of those contained in the last Table, gives the following proportions:—

Age on Admission.	Total.	Heart Affected.	Per cent.
Under 10 years	0	0	0·0
Over 10 under 15	3	3	100·0
„ 15 „ 20	10	6	60·0
„ 20 „ 25	11	8	72·7
„ 25 „ 30	6	3	50·0
From 30 to 40	16	7	43·7
Over 40 years	7	3	42·9

This Table, compared with one at page 101, containing the ages of all the acute cases on admission, shews the additional ratio of cardiac affection for each period, due to renewed attacks of rheumatism. The general fact which they all indicate is, that up to 25 years of age, there is a much greater liability to disease of the heart following on acute rheumatism. But this is less in cases of a repeated attack, not only relatively to more advanced ages, but absolutely in proportion to the number of cases.

3. *Of a History of Acute Rheumatism.*

In addition to the record of previous attacks in acute cases, an attempt has been made in all the rheumatic cases[1] together, to ascertain how many had at any time suffered from acute rheumatism, and to note the condition of the heart. In 450 instances this record has been kept, when it appeared that 200 had had previous attacks of some sort or other, while 250 had never suffered from it before to their knowledge. In 88, so far as could be made out, the attack was acute, and 32 had disease of the heart, only 2 being marked as doubtful. In almost all, the date of the first attack has been recorded.

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[1] In making this inquiry it was to be remembered, how difficult it is to obtain correct information of this sort from Hospital patients, and it therefore seemed desirable to limit it to persons who, being at the time subjects of rheumatism, might the more readily answer whether they had ever been before similarly affected, while there would be in the condition of the patient himself at the time, a standard of comparison with which to contrast his own account of his previous state. It was necessary in this case to adopt the limitation of acute

ADMITTED WITH SUB-ACUTE RHEUMATISM.

Age at First Attack.	Rheumatism not Acute.	Acute Rheumatism.	Heart Diseased.	Per cent.
Under 10 years	0	3	3	100·0
Over 10 under 20	6	16	8	50·0
„ 20 „ 30	9	18	7	38·9
„ 30 „ 40	6	7	0	0·0
Over 40 years	7	2	0	0·0

ADMITTED WITH CHRONIC RHEUMATISM.

Age at First Attack.	Rheumatism not Acute.	Acute Rheumatism.	Heart Diseased.	Per cent.
Under 10 years	0	1	0	0·0
Over 10 under 20	4	14	6	42·8
„ 20 „ 30	27	13	5	38·5
„ 30 „ 40	19	12	3	25·0
Over 40 years	34	2	0	0·0

Much reliance cannot, I fear, be placed on these proportions, although every effort has been used to obtain accurate results. If they be worth anything at all, they give a very favourable view of the ultimate progress of cases of acute rheumatism, especially where there has been no recurrence of the disease in an acute form.

4. *Confirmed Disease of the Heart.*

In addition to 82 cases of disease of old standing to be found in the Table of *post-mortem* appearances, there are records of 124 which left the hospital more or less relieved;

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rheumatism already noted, and a useful adjunct was found in the question whether the person had been at any time unable to feed himself, and for how long. There seemed also to be an advantage in selecting a class of cases in so far similarly circumstanced, for statistical purposes, in preference to making the same inquiry of every patient for a limited time. The conclusions are open to the fallacy already noticed, that disease of the heart does sometimes occur, although very rarely in a subacute attack; but the question was always put before the heart was examined, and the result was that in only two instances, as already noticed, the heart was affected with a history of merely slight rheumatism. The ratio of diseases of the heart has, therefore, only been given for acute cases.

206 in all. Among these there is a distinct history of acute rheumatism in 69 instances; but in 4 instances, from *post-mortem* appearances, and in 3 from clinical examination, there was reason to doubt whether the lesion could have been the result of rheumatic inflammation. Such cases must be regarded as doubtful, as well as those having a history of only slight rheumatism or gout, for though on reference to the Table it will appear that in the majority the two diseases are merely accidentally coincident, still to this there are exceptions. One has been already mentioned (p. 100); another stated that he had suffered from gout at 18, which he alleged to be hereditary; but *post-mortem* evidence shewed the disease to have had an inflammatory origin, whence the presumption is that the attack was of the nature of acute rheumatism rather than gout. To these doubtful cases must be also added all in which no record on this point has been kept; for in reference to such a question negative results are as important as positive ones; and it was my intention to record both, the omission having chiefly occurred in cases affording no evidence of anything beyond alteration in dimensions, which the mind does not naturally refer to rheumatism as one of its causes.

The whole number recorded as never having suffered from rheumatism at all was 73: and if from the rheumatic cases all questionable ones be excluded together, there will remain 62 rheumatic, and 71 doubtful.

In considering the influence of sex, it is necessary to bear in mind, that taking all medical cases together at St George's Hospital, the proportion of admissions and deaths for each sex is nearly equal throughout the year. Thus, for example, the physicians' patients admitted during six months ending in March 1851, were 347 males and 344 females, while the deaths during the same period were 41 males and 41 females. In confirmed disease of the heart, there is a very decided preponderance of the male sex, there being 119 males and 87 females. The per-centage of mortality is also higher, being about 45 for males and 36 for females. This is the more remarkable, as we have found females somewhat more liable than males to disease of the heart from acute rheumatism. The relation of sex to a rheumatic origin may be represented thus for the 206 cases under consideration:

Males.

Of rheumatic origin	34 or 16·5 per cent.
Non-rheumatic „	41 or 20·0 „
Doubtful „	45 or 21·8 „

Females.

Of rheumatic origin	28 or 13·6 per cent.
Non-rheumatic „	32 or 15·5 „
Doubtful „	26 or 12·6 „

Whence it appears, that although this preponderance does *not* exist to the same extent in cases decidedly rheumatic, still, even in those there is an excess of males admitted labouring under confirmed lesion.

The influence of age with reference to a rheumatic origin seems to be very considerable. The cases may be arranged, according to their ages at admission, in periods of ten years.

Age on Admission.	Of Rheumatic Origin.		Non-Rheumatic		Doubtful.	
	Fatal.	Not Fatal.	Fatal.	Not Fatal.	Fatal.	Not Fatal.
Under 10 years . .	0	2	0	1	0	1
Over 10 under 20 .	4	17	0	1	1	2
... 20 ... 30 .	4	10	5	8	1	7
... 30 ... 40 .	4	7	5	13	7	7
... 40 ... 50 .	4	5	13	6	13	7
... 50 ... 60 .	2	1	7	5	10	5
Over 60 years . .	0	0	8	2	1	10

The striking fact here is, that up to the age of 20 almost the whole, and even as far as 30, considerably more than half the cases, are ascribed to a rheumatic origin; between 20 and 30 the non-rheumatic cases first appear; in the two succeeding periods they nearly double the rheumatic; and after 50 there are scarcely any rheumatic cases at all. The ratio of mortality sensibly increases in each period, beginning with one-fifth of all the cases between 10 and 20, and reaching to two thirds between 50 and 60. I am quite unable to explain the anomaly of only one death among 11 cases in the last colnmn, over 60 years of age.

In 59 of the cases supposed to be of rheumatic origin, the age at the period of the first attack has been recorded, and is here given for comparison with preceding Tables:

Age at First Attack.	Males.	Females.	Total.
Under 10 years	3	5	8
Over 10 under 15	11	5	16
„ 15 under 20	4	8	12
From 20 to 30	13	1	14
„ 30 to 40	3	4	7
Over 40 years	1	1	2

Among the fatal cases of rheumatic origin, the period from the first attack of acute rheumatism to the fatal termination, may afford some evidence of the general duration of cardiac disease; but the instances are very much too few to form the ground of anything but the most general conclusions. Recent cases must be excluded, and also those doubtful ones of which the commencement in rheumatism was uncertain. As might be anticipated, the laborious occupations of the male sex among hospital patients materially shortens their duration; thus there are 2 deaths at 4 years after the first attack of acute rheumatism, 2 at 5, 2 at 6, 1 at 8, and 1 at 23, as well as 1 at 30 years after. It is unfortunately not recorded in either of these last instances whether there had been any subsequent attacks. Among females there are 2 deaths at 12 years, 1 at 13, 2 at 15, and 1 at 16 years after their first attack.

In conclusion, I may be permitted to recapitulate the several results of this Statistical Inquiry. Of 8 cases fatal during the rheumatic attack, 6 had pericarditis, 3 endocarditis, and 2 no inflammation of the heart. One case of pericarditis gave no friction sound, and one case with no valvular lesion gave a distinct endocardial murmur.

Two other cases of pericarditis are traced to acute peritonitis, and one to recent pleurisy. A large proportion of the remainder were associated with albuminuria; but it has seemed in these instances that the tumultuous action of the heart during life had had very much to do with the traces of pericarditis found after death. In one instance there was no other cause known.

Two cases of recent lymph on the valves are traced to malformation, and two to disease of the kidney; almost all the remaining instances occurred among patients in whom old thickening was found, which was probably due to bygone attacks of rheumatism.

More than half the cases which had at one time suffered from acute rheumatism indicate, old pericarditis. The pericardium was found six times universally firmly adherent, in only one of which there was a history of rheumatism; in three, the adhesions were nearly universal with histories of acute rheumatism ; and about a third of the remainder with only slight or no adhesions were also rheumatic : hence it would appear that universal adhesion is not the common result of rheumatic pericarditis, nor does the condition of the heart in these cases appear to indicate that it is the most favourable termination.

About a third of the cases of valvular lesion are associated with a rheumatic history, and among them there is a great preponderance of cases of simultaneous lesion of both sets of valves, next of the mitral alone, and scarcely any in which the aortic valves alone are diseased. It seems in most of these cases probable that during the first attack of rheumatism the mitral valves were alone inflamed, and at some future period also the aortic secondarily. In more than half the cases of valvular lesion the kidney was not quite healthy.

Disease of the aortic valves is more frequently associated with hypertrophy, and disease of the mitral valve with dilatation ; adhesions of the pericardium mostly with dilatation. Atheroma of the aorta and disease of the kidney, each accompany a large majority of the cases of simple hypertrophy. In nearly half of all cases of disease of kidney indiscriminately, there is alteration in the muscular structure of the heart.

In regard to acute rheumatism, it is shewn that in the greater number of instances it occurs before 25 years of age, and that the largest proportion of cardiac inflammations are found between 10 and 15 years of age. Females are in a slight degree more liable to it than males, and are also more subject to cardiac complications. On the other hand, there are many more males than females admitted with confirmed disease of the heart, but the difference is less when the disease is of rheumatic origin. At the earlier ages almost all cases of confirmed disease of the heart owe their origin to acute rheumatism ; at more advanced ages it almost ceases to be found among the causes.

Instead of giving the tabular form of the cases, it has been thought most useful to present them in the shape of short abstracts, containing, however, all the points given in the tables. In these abstracts, P. means Pericardium ; H. the Heart ; A. Aortic Valves ; M. Mitral Valve ; R. Rheumatism ; C. Chambers of Heart ; K. Kidneys.

1. Male, aged 12. P. thick layer of recent lymph ; adhesions ; membrane thickened. A. recent lymph. M. thickened recent lymph. R. on admission and six years ago. Tricus-

pid thickened; recent pleuritis and pneumonia. Acute rheumatism of fifteen days' duration; murmur at base and apex, and friction sound.

2. Female, aged 17. P. recent lymph; firm adhesions; numerous shreds of lymph. H. hypertrophied. C. dilated. A. recent lymph. M. thickened and recent lymph. R. on admission, and previously. Recent pleuritis; capsule of liver thick. Third attack of acute rheumatism; friction sound, and an indistinct systolic murmur.

3. Male, aged 21. P. recent pericarditis; much serum. R. on admission. Acute rheumatism of seven days' duration; collapse on admission.

4. Female, aged 30. P. thin layer of recent lymph. H. fatty. M. recent lymph. R. on admission. Acute rheumatism of twelve days' duration; friction sound.

5. Female, aged 41. P. recent lymph and old white patch. M. slightly atheromatous. R. on admission. Enlarged Peyer's glands; congested lungs; atheroma of aorta. First attack of acute rheumatism, fatal in twenty-two days; no friction sound ever heard, rhythm altered. (Continued fever)?

6. Male, aged 25. P. very slight effusion of lymph. R. on admission. Suppuration in and round joints; congestion of brain. Acute rheumatism (?) of thirteen days' duration; suppuration, delirium.

7. Female, aged 23. R. on admission. Small quantity of turbid serum in peritoneum. Acute rheumatism; delirium; systolic murmur above base of heart.

8. Female, aged 45. A. opaque. M. opaque. R. on admission. Pus in synovial cavities of both wrists. Moribund; treated before admission for rheumatic fever; collections of matter at wrists.

9. Male, aged 16. P. numerous shreds of lymph. R. never (?) Recent peritonitis and pleuritis. Peritonitis from injury (?), rapidly fatal.

10. Female, aged 46. P. Feebly adherent; recent lymph. H. hypertrophied. C. dilated. R. never. K. granular. Recent peritonitis and pleuritis; cirrhosis of liver. Dropsy, especially ascites; heart's action extended; dyspnœa; albuminuria; peritonitis, &c., occurring after paracentesis abdominis.

11. Male, aged 42. P. thick layer of recent lymph. Extensive recent pleuritis and pneumonia. Pleuro-pneumonia; heart's sounds distant and obscure.

12. Female, aged 15. P. coated with effused lymph. H. hypertrophied. R. eight months ago (?). K. right mottled; left, dwindled. Recent pleuritis; serous effusion in brain. Albuminuria; facial hemiplegia; pericardial friction.

13. Female, aged 38. P. few shreds of lymph. H. slightly atrophied. C. dilated. M. greatly thickened. R. First attack twenty-six years ago. K. puckered, cysted. Recent lymph on tricuspid, and thickening; old pleuritis; capsule of liver thickened. Dropsy: systolic murmur at apex of heart; very little albumen in urine.

14. Female, aged 33. P. recent pericarditis. H. hypertrophied. A. atheromatous. M. atheromatous. R. never. Granular. Atheroma of aorta; slight recent pleuritis; nutmeg liver. Dropsy; dyspnœa; heart's action increased, systolic murmur at apex; urine very albuminous; diffuse cellular inflammation.

15. Female, aged 40. P. much recent shreddy lymph. H. slightly hypertrophied. K. contracted granular. Liver enlarged, nutmeg; atheroma of aorta. Albuminuria and enlarged liver; pericarditis supervening before death.

16. Female, aged 40. P. some shreds of lymph; large quantity of serum. H. hypertrophied. R. never. K. cysted, congested. Lateral curvature of spine; large nutmeg liver. Dropsy; loud systolic murmur; albuminuria.

17. Female, aged 44. P. small fringe of recent lymph. M. thickened and recent lymph. R. never. K. dilated cyst. Scirrhus of uterus and vagina pressing on ureters. Anasarca, albuminuria, cancer of uterus, &c.; heart's sounds obscure, apparently a double murmur.

18. Male, aged 25. P. few flakes of lymph. H. hypertrophied, flabby. C. greatly dilated. A. malformation; recent lymph. M. recent lymph. R. never. Tricuspid and pulmonic valves atrophied; lymph on endocardium; nutmeg liver. Dropsy; dyspnœa; heart's action tumultuous; no bruit made out; hæmoptysis.

19. Male, aged 35. P. few flakes of lymph. H. hypertrophied. C. greatly dilated. A. greatly thickened. M. opaque, slightly thick. R. occasional slight, never acute. Lungs congested, rather emphysematous, commencing peritonitis. Cough and palpitation, never previously; heart's action increased; double murmur at base; latterly dropsy.

20. Male, aged 32. P. Several patches of recent lymph, large old white spot. C. dilated. A. slightly thickened. M. thickened, contracted. R. first attack, six years ago. Tricuspid thickened. Dropsy; heart's action increased; loud systolic murmur at apex; slight murmur at base (?); urine transiently albuminous.

21. Male, aged 40. P. few shreds of lymph; two white patches. H. slightly hypertrophied. C. dilated. A. thickened, calcareous, recent lymph. R. first attack five years

ago. Tricuspid slightly thickened; old pleuritis. Chronic rheumatism; double murmur at base of heart; anasarca; dyspnœa.

22. Male, aged 45. P. firm, thick, false membrane; patches of shreddy lymph. C. dilated. A. opaque. M. thickened. R. six months ago. K. cysted. Aneurism of aorta. Dropsy; heart's action irregular; no albumen in urine.

23. Female, aged 43. P. layer of recent lymph; gray patch on heart. H. hypertrophied. K. granular, cysted. Atheroma of aorta. Hæmoptysis; œdema of ankles; heart's action increased, sounds indistinct; albuminuria; friction before death.

24. Male, aged 50. P. partial adhesions; not quite recent lymph. H. greatly hypertrophied. C. dilated. M. recent lymph. R. first attack six years ago. K. dwindled, adherent capsules. "Severe cold" four months before; dropsy; dyspnœa; heart's sounds muffled and obscure; albuminuria.

25. Male, aged 54. P. universally, not firmly, adherent. H. slightly hypertrophied. C. slightly dilated. A. thickened. M. thickened, calcareous. R. first attack thirty years ago. K. granular. Slight old pleuritis; extensive old peritonitis. Ascites and anasarca; albuminuria; heart's action irregular; obscure systolic murmur.

26. Male, aged 58. P. universally adherent, rather recent lymph. H. slightly hypertrophied. C. dilated. R. twenty-five years ago. Atheroma of aorta; tubercles and consolidation of lungs; liver rounded, nutmeg. "Severe cold" six months ago; palpitation four months; heart's action irregular; phthisis; anasarca; no illness for twenty-five years past.

27. Female, aged 35. P. chronic thickening, and rather recent lymph. H. partially atrophied. C. greatly dilated. M. opaque. R. never. Tricuspid opaque; peritonitis old and tolerably recent. Dropsy; heart's action extended; urine transiently albuminous; local peritonitis two months before death.

28. Male, aged 25. H. hypertrophied. A. one flap large, recent lymph. R. never. K. large, smooth, mottled. Old peritonitis; atheroma of aorta. Double murmur at base of heart, with increased action; general dropsy; no illness for twenty years.

29. Male, aged 45. P. chronic thickening of membrane. H. hypertrophied. C. dilated. A. one spot of recent lymph. R. never. K. granular, cysted. Old pleuritis and peritonitis; nutmeg liver. Dropsy; heart's action increased; albuminuria.

30. Female, aged 16. P. considerable old adhesions. H. hypertrophied. C. dilated. M. contracted and recent lymph. R. first attack thirteen years ago. Lymph on endocardium; atheroma of aorta; congestion of lungs. Cough and palpitation; dropsy after admission; heart's action increased; loud systolic murmur at apex.

31. Male, aged 20. P. almost universal firm adhesions. H. greatly hypertrophied. C. dilated. A. recent lymph. M. thickened recent lymph. R. first attack four years ago. K. large, congested. Old pleuritis. Dropsy; moribund on admission; heart's action tumultuous; murmur at apex; albuminous urine.

32. Female, aged 37. H. hypertrophied. C. dilated. A. thickened, calcareous, recent lymph. M. greatly thickened, calcareous. R. first attack twenty-two years ago K. extravasated blood. Old and recent pleuritis. Dropsy; heart's action increased, systolic murmur at base and apex; urine very albuminous; hæmoptysis.

33. Female, aged 38. P. general old adhesion. H. attenuated. C. dilated. A. thickened. M. thickened recent lymph. R. first attack twenty-one years ago. Old pleuritis; inflamed bronchi; old peritonitis; nutmeg liver. Three times in hospital with anasarca and ascites; double murmur at base of heart.

34. Male, aged 42. P. large patch of old lymph. H. greatly hypertrophied. C. dilated. A. thickened recent lymph. M. greatly thickened. R. first attack twenty-three years ago. Twice previously in hospital with cardiac pain and spasm; slight anasarca; heart's action increased, murmur at apex and base (?).

35. Male, aged 17. P. universally firmly adherent. M. not quite recent lymph. R. five years ago, and just before admission (?). Inflammation of spinal cord. Chorea; heart's sounds excited and indistinct; systolic murmur at apex.

36. Female, aged 42. M. not quite recent lymph. R. six months ago. Tolerably recent lymph on lining membrane of left auricle. Dyspnœa; heart's action excited and irregular; systolic murmur at apex.

37. Male, aged 33. P. universal old adhesion; adherent pericardium. H. greatly hypertrophied. C. dilated. A. thickened calcareous deposit. K. right atrophied. Congenital malformation of aorta; old pleuritis; nutmeg liver. Pain in region of liver; dyspnœa; double murmur at base of heart; hæmoptysis.

38. Female, aged 40. P. universal old adhesions. H. hypertrophied. M. slightly roughened. R. never. Old and

recent peritonitis; old pleurisy on right side. Ascites with anasarca; paracentesis twice in four months; peritonitis followed on second operation.

39. Male, aged 43. P. universally firmly adherent. R. never (?). K. small. Cirrhosis of liver. Ascites with anasarca; no albuminuria, no cardiac symptoms; no previous illness.

40. Male, aged 46. P. universal old adhesions. H. pale, atrophied. C. dilated. M. thickened, atheromatous. R. never. K. coarse, granular. Old and recent pleuritis; nutmeg liver. Dropsy; heart's action feeble, murmur at apex; no albumen observed in urine.

41. Male, aged 55. P. universal old adhesion. H. greatly hypertrophied. C. greatly dilated. R. never. K. somewhat dwindled. General dropsy, dyspnœa, hæmoptysis; heart's sounds muffled, irregular; urine transiently free from albumen.

42. Male, aged 22. P. membrane thickened; large old patch of lymph. H. slightly hypertrophied. C. greatly dilated. A. thickened. M. thickened, calcareous deposit. R. eight years ago. Emphysema of lungs; pleuritis not quite recent. Excessive dyspnœa; pulse 240 (?); heart's action very greatly excited; sounds imperfect.

43. Female, aged 27. P. considerable old adhesions, and large white spot. H. slightly hypertrophied. C. greatly dilated. M. thickened. R. fifteen years ago. Nutmeg liver. Dropsy; heart's action irregular; murmur at apex.

44. Male, aged 26. P. considerable firm adhesions. H. greatly hypertrophied. C. greatly dilated. A. slightly thickened. M. slightly thickened. R. never. Old pleuritis on left side. Dropsy; heart's action increased; systolic murmur at apex.

45. Male, aged 45. P. several white patches. A. thickened, atrophied. R. never. K. right, puckered, congested. Atheroma of aorta. Dropsy; hæmoptysis; double murmur at base of heart; urine transiently albuminous.

46. Female, aged 46. P. whitish membranous layer. H. slightly hypertrophied. C. dilated. Old pleuritis and peritonitis; nutmeg liver. Dropsy; heart's action irregular, feeble.

47. Male, aged 50. P. partial old adhesions. C. aneurismal dilatation. R. never (?). Lining membrane of aorta thickened. Dropsy; double murmur over centre and base of heart.

48. Male, aged 51. P. several white patches. H. attenuated. C. dilated. Emphysema and bronchitis; slight old

pleuritis; vomicæ, and few crude tubercles. Excessive dyspnœa; anasarca; heart's sounds loud; peritonitis, &c.

49. Male, aged 55. P. firm band of old adhesion. H. slightly hypertrophied. C. dilated. A. ulcerated. R. never. K. dwindled, capsules adherent. Tricuspid thickened; atheroma of aorta; recent pleuritis; nutmeg liver. Dropsy; dyspnœa; albuminuria; heart's action irregular; murmur, with second sound at base.

50. Male, aged 56. P. large patch of old lymph. H. greatly hypertrophied. C. greatly dilated. A. thickened. M. slightly thickened. K. granular, cysted. Atheroma of aorta. Dropsy; hæmoptysis; albuminuria; heart's action increased; double murmur at base.

51. Male, aged 73. P. two white patches; slight adhesions. H. hypertrophied. C. dilated. A. thickened, atheromatous. M. thickened, atheromatous. R. never. K. cysted, adherent capsules. Atheroma and ulceration of aorta; old pleuritis and peritonitis. Dropsy; urine very albuminous; heart's action feeble, tumultuous; obscure systolic murmur.

52. Male, aged 24. H. hypertrophied. C. dilated. M. thickened, contracted. R. four years ago. Recent pleuritis, from pulmonary apoplexy (?). Slight dropsy; great dyspnœa; heart's action much increased; musical murmur at apex.

53. Female, aged 42. H. slightly atrophied. C. dilated. M. slightly thickened. R. many years ago. K. large, granular. Tricuspid thickened; atheroma of aorta; extensive old pleuritis. Dropsy; heart's action irregular; murmur at apex; no albumen in urine.

54. Female, aged 51. H. slighted hypertrophied. C. greatly dilated. A. slightly thickened. M. thickened. R. first attack fifteen years ago. K. granular, cysted. Old pleuritis; capsule of liver thickened. General dropsy; albuminuria; heart's action irregular; systolic murmur most distinct at apex.

55. Female, aged 44. H. fatty, hypertrophied. C. dilated. A. opaque. M. opaque. Slight rheumatism seven months ago. K. large, coarse. Pneumonia. Dropsy; heart's action irregular; albuminuria.

56. Male, aged 32. H. greatly hypertrophied. M. thickened, contracted, and atheromatous. R. gout (?) fourteen years. Heart's sounds loud and shrill; urine albuminous; dropsy; sphacelus after acupuncture.

57. Male, aged 40. H. greatly hypertrophied. A. deposit of lithate of soda. R. Hereditary gout twenty years. K.

granular. Recent pleuritis; pneumonia; liver large, rounded; various deposits of lithate of soda. Epistaxis; albuminuria; pleuro-pneumonia.

58. Male, aged 54. H. hypertrophied. C. dilated. A. slightly atheromatous. M. slightly atheromatous. R. slight rheumatism thirty-one years ago. K. granular, cysted. Atheroma of aorta; old pleuritis; capsule of liver thickened. General dropsy; albuminuria; heart's sounds obscure; no bruit.

59. Male, aged 22. H. hypertrophied. C. greatly dilated. A. slightly thickened. M. atheromatous. R. never. K. congested. Atheroma of tricuspid; old and recent pleuritis. Dropsy; dyspnœa; heart's action tumultuous, murmur at apex; urine highly albuminous.

60. Male, aged 45. H. hypertrophied. A. atheromatous. R. never. Aorta dilated, roughened with atheromatous and calcareous deposit. Dropsy; double aortic murmur.

61. Male, aged 46. P. small patch of old lymph. H. pale. A. atheromatous. M. atheromatous. R. never. Atheroma of tricuspid, of endocardium, and aorta; liver large, fatty. Jaundice; ascites; delirium; very dissipated habits; no cardiac bruit.

62. Female, aged 49 H. slightly hypertrophied. C. dilated. A. slightly atheromatous. M. slightly atheromatous. R. never. Old and recent pleuritis; liver congested, capsule thickened. Dropsy; heart's action feeble; died suddenly.

63. Male, aged 54. H. greatly hypertrophied. C. slightly dilated. A. slightly atheromatous. K. granular, mottled. Atheroma of aorta. Dropsy; hæmoptysis; albuminuria; heart's action increased; double murmur at base.

64. Male, aged 57. H. slightly hypertrophied. A. calcareous. M. calcareous. Calcareous deposit in aorta; bronchitis; liver contracted, granular. Dropsy and bronchitis; dyspnœa; heart's action quiet; no bruit.

65. Female, aged 58. H. greatly hypertrophied. C. dilated. A. atheromatous. M. atheromatous. R. never. Atheroma of aorta; old pleuritis. Dropsy; hæmoptysis; heart's action increased; double murmur at base, systolic at apex.

66. Female, aged 64. H. slightly hypertrophied, fatty. C. dilated. M. thickened, calcareous. R. never. K. large, mottled. Organs all loaded with fat; liver contracted, granular. Anasarca; lividity of skin; no albumen in urine; heart's action very feeble; murmur at apex.

67. Male, aged 66. P. white patch. H. greatly hypertrophied. A. slightly atheromatous. M. slightly athero-

matous. K. granular, cysted. Atheromatous deposit in coronary arteries; emphysema of lungs; thickened capsule of liver. Dropsy; albuminuria; heart's action irregular; indistinct systolic murmur.

68. Female, aged 68. P. small white patch. H. hypertrophied. C. dilated. M. thickened, atheromatous. R. never. K. rough, adherent capsules. Old pleuritis. Dropsy; hæmoptysis; no albumen in urine; heart's action excited; no bruit.

69. Male, aged 52. P. two small white patches on heart. H. greatly hypertrophied. C. dilated. R. slight rheumatism many years ago. K. granular. Atheroma of aorta; old pleuritis. Slight chronic rheumatism; constant vomiting; albuminuria.

70. Female, aged 35. H. hypertrophied. C. dilated. M. slightly thickened. K. granular. Old and rather recent pleuritis; liver large, congested. Anasarca; albuminuria; heart's action tumultuous; murmur at apex.

71. Male, aged 36. C. greatly dilated. A. slightly thickened. M. slightly thickened. R. never. K. coarse, lobulated. Nutmeg liver. Dropsy; hæmoptysis; heart's action turbulent; no albumen in urine; of intemperate habits.

72. Male, aged 53. H. slightly hypertrophied. C. greatly dilated. M. thickened. R. never. Tricuspid thickened; atheroma of aorta. Dropsy; heart's action irregular; murmur at apex.

73. Female, aged 60. H. fatty degeneration. C. dilated. A. opaque. M. opaque. R. never (?). K. granular, cysted. Anasarca; heart's action feeble; sounds indistinct; albuminuria.

74. Male, aged 65. H. right especially hypertrophied. C. right side dilated. M. thickened. R. never. K. coarse, mottled, cysted. Calcareous deposit in aorta; emphysema of lungs; capsule of liver puckered. Dropsy; dyspnœa; heart's sounds prolonged; urine transiently free from albumen.

75. Female, aged 75. P. small white patch. H. slightly hypertrophied. M. slightly thickened R. never. K. cysted. Atheroma of aorta; old pleuritis; capsule of liver thickened. Dropsy; heart's action very irregular; urine transiently albuminous.

76. Male, aged 35. P. small white patch. H. hypertrophied. C. greatly dilated. M. slightly thickened. R. never. K. granular, cysted. Old and recent pleuritis. Dropsy; pleuro-pneumonia; heart's action tumultuous; murmur at apex; urine not albuminous (only one examination).

77. Male, aged 43. C. greatly dilated. M. much thickened, contracted. R. never. K. capsules adherent. Aneurism from atheroma and calcareous deposit in aorta. Dyspnœa; double murmur over aortic valves.

78. Male, aged 31. A. atrophied, perforated. Recent pleuritis; emphysema. Pleurisy and bronchitis; no cardiac murmur.

79. Female, aged 40. H. slightly hypertrophied. C. slightly dilated. A. much thickened; atheromatous. M. thickened, contracted. K. small, pale, mottled. Atheroma of aorta. Slight jaundice, followed by dropsy; urine for some time free from albumen; heart's action tumultuous; no bruit heard; latterly albuminuria.

80. Male, aged 34. H. hypertrophied. C. greatly dilated. Only slight rheumatism. K. granular, cysted. Old pleuritis; contracted nutmeg liver. Dropsy; heart's action increased; murmur at apex; urine transiently albuminous.

81. Male, aged 45. H. hypertrophied. R. gout, nineteen years. K. atrophied, granular. Dropsy; albuminuria.

82. Female, aged 46. C. dilated. Slight rheumatism seventeen years ago. K. mottled, cysted. Old pleuritis; emphysema of lungs. Dropsy; albuminuria; heart's action excited.

83. Male, aged 51. H. hypertrophied. R. gout twenty years. K. granular, cysted. Old pleuritis; miliary tubercle. Dropsy; dyspnœa; epistaxis; albuminuria.

84. Male, aged 24. H. right side hypertrophied. C. right side dilated. R. never. Great emphysema and bronchitis; old pleuritis; liver large. Great dyspnœa; slight anasarca; heart's action turbulent, feeble.

85. Male, aged 29. H. hypertrophied. C. dilated. Extensive old peritonitis; liver much enlarged, nutmeg; pneumonia. Ascites; heart's sounds obscure.

86. Male, aged 39. H. slightly hypertrophied. C. greatly dilated. Pleuritis not quite recent. Pleuro-pneumonia six months before death; dropsy; increased impulse of heart.

87. Female, aged 40. H. hypertrophied. C. dilated. K. mottled. Liver nutmeg, with rounded edges. Dropsy; heart's action increased; albuminuria.

88. Male, aged 44. H. hypertrophied. C. dilated. K. congested, adherent capsules. Pleuritis, old and recent; liver friable. Dropsy; great dyspnœa; heart's action irregular; urine slightly albuminous.

89. Male, aged 45. H. greatly hypertrophied. K. granular, atrophied. Atheroma of aorta; slight old pleuritis; apoplectic clot in ventricle of brain. Dropsy two years ago;

albuminuria; heart's action apparently very little increased; apoplexy occurring in hospital.

90. Female, aged 47. H. hypertrophied. C. dilated. R. never. K. granular, cysted. Calcareous deposit in aorta; nutmeg liver. Dropsy; albuminuria; heart's sounds shrill.

91. Male, aged 71. C. dilated. R. never. K. right large; left dwindled. Endocardial membrane thickened; old pleuritis. Anasarca; albuminuria; palpitation; heart's sounds indistinct.

92. Female, aged 54. H. slightly hypertrophied. C. greatly dilated. R. never. K. congested, granular. Bronchitis and emphysema of lungs. Cough and palpitation three years; anasarca; albuminuria; heart's action tumultuous; systolic murmur at apex.

Art. IX.—*On some of the principal Effects resulting from the Detachment of Fibrinous Deposits from the Interior of the Heart, and their Mixture with the Circulating Blood.*

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That the fibrinous principle of the blood may, under certain circumstances, separate from the circulating fluid during life, and be deposited within the vascular system, especially on the valves of the heart, is a fact so clearly established and so generally admitted, that I need only, at the outset of the communication I have the honour to present to this Society, allude to it as a settled truth, and refer, for the proofs, to the various general works on diseases of the heart and blood-vessels, and to such special essays on the subject as those of Dr Burrows[1] and Dr Hughes.[2] From these sources may also be gathered nearly all that is yet known respecting the various conditions under which the deposition of fibrin takes place, and the several forms which the deposits assume. Into these general details I do not purpose entering, my object being simply to consider the effects which the deposits may produce on the system at large. It may, however, be premised that the forms of fibrinous concretions to which my observations chiefly apply, are, first, the masses usually described as Laennec's globular excrescences; and, secondly, the granular or warty growths adhering to the valves, and presenting innumerable varieties, from mere granules to large irregular fungous or cauliflower excrescences projecting into the cavities of the heart.

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[1] Med. Gaz., vol. xvi., 1834–5.

[2] Guy's Hosp. Reports, vol. iv., 1839.

Avoiding all discussion concerning the origin of these latter growths, I proceed at once to state, that in whatever way they may originate, they are, when once formed, full of peril, and often remain so even long after the circumstances which gave rise to them have passed by. If of large size and only loosely-adherent, as they often are, one or more masses of even considerable magnitude may at any time be detached from the valves and conveyed with the circulating blood until arrested within some arterial canal which may be completely plugged up by it, and thus the supply of blood to an important part be suddenly cut off, and serious, even fatal results ensue. Or, the deposits on the valves may be detached in smaller masses, and pass on into arteries of much less size, or even into the capillaries, where, being arrested, they may cause congestion, followed by stagnation and coagulation of the blood, with all the subsequent changes which blood coagulated within the living body is liable to undergo. In this way are probably induced many singular morbid appearances often observed in internal organs, and rarely well accounted for. Again, the masses of fibrin may soften, break up, and discharge the finely granular material resulting from their disintegration; and this, mingling and circulating with the blood, may give rise to various disturbances indicative of a contaminated state of this fluid, producing symptoms very similar to those observed in phlebitis, typhus, and other analogous blood-diseases. In one or more of these several ways, and probably in others not yet clearly recognised, fibrinous material detached from the valves, or any other part of the interior of the heart, may be the cause of serious secondary mischief in the body.

It appears unnecessary to insist here on the possibility of any of the various forms of fibrinous deposit found within the heart being detached either spontaneously or by the mere force with which the current of blood passes over the surfaces on which they are placed. For it is well known that after death a very gentle force, sometimes even the slightest touch, will loosen and dislodge both small granular particles and masses of considerable size from the valves and inner surface of the heart. Not unfrequently, indeed, lumps of old laminated fibrin of even considerable magnitude are found loose in the cavities of the heart, having probably dropped off before death; and sometimes a mass of this kind may be found some distance along the aorta or pulmonary artery.

It is clear, then, that such fibrinous deposits may admit of being very readily detached, and it must be equally clear that once floating freely in the blood they are exposed to the almost certain consequence of being transmitted with this fluid, and stopped at the first vessel too narrow to allow of their transit.

The parts of the vascular system within which these transmitted masses of fibrin may be found will of course depend, in great measure, upon whether they proceeded from the right or left side of the heart. Thus, if they have been detached from either the aortic or mitral valves, they will pass into the blood propelled by the left ventricle into the aorta and its subdivisions, and may be arrested in any of the systemic arteries, or their ramifications in the various organs, especially those which, like the brain, spleen, and kidneys, receive large supplies of blood directly from the left side of the heart.

If, on the other hand, the fibrinous masses are derived from the pulmonary or tricuspid valves, the pulmonary artery and its subdivisions within the lungs will necessarily become the primary if not the exclusive seat of their subsequent deposition. A division of the subject being thus naturally formed, I propose to embody the remarks I am about to submit to the Society under two principal heads, considering;—

1*st*, The remote effects resulting from the separation of fibrinous or analogous deposits from the valves or interior of the left side of the heart; and,

2*d*, The corresponding effects produced by the detachment of like deposits from the valves or interior of the right side of the heart.

PART I.

On the effects which may result from the separation of fibrinous deposits from the valves or interior of the left side of the Heart, and their circulation with the systemic blood.

In endeavouring to elucidate this part of the subject, I beg to draw attention, in the first place, to instances in which it seems probable that masses of considerable magnitude have been detached from the left side of the heart, and subsequently arrested in an arterial channel of notable size; secondly, to some of the effects which seem to ensue when smaller arterial vessels or capillaries are similarly blocked up; and, thirdly, to circumstances which make it probable that, not unfrequently, the introduction of particles of fibrin into the circulating blood gives rise to constitutional symptoms indicative of a poisoned state of this fluid,

I. The first three cases which I shall offer are in many respects identical; for in each, death seemed to ensue from softening of the brain, consequent on obliteration of one of the main cerebral arteries by a mass of fibrinous material, apparently derived directly from warty growths on the left valves of the heart.

Case 1.—Margaret Shaw, aged 34, a pale, weakly-looking woman; admitted into St Bartholmew's Hospital, under Dr Roupell, about the middle of July 1850, on account of pains in her lower limbs, and general debility. A loud systolic murmur was heard all over the cardiac region. No material change ensued in her condition until August 7th, when, while sitting up in bed eating her dinner, she suddenly fell back as if fainting, vomited a little, and when attended to was found speechless, though not unconscious, and partially hemiplegic on the left side. The hemiplegia increased, involving the left side of the face as well as the limbs, and gradually became complete in regard to motion, while sensation seemed to remain unimpaired. She continued speechless and hemiplegic, but without loss of consciousness, for five days, when she quietly died.

On examining the body, six hours after death, the skull and dura mater were found natural; but the small vessels of the pia mater were much congested, the congestion amounting, in some places, almost to ecchymoses. The right corpus striatum was softened to an extreme degree, being reduced to a complete pulp of a dirty grayish-white tint, and without any remains of its characteristic striated structure. The corresponding optic thalamus was healthy; but a condition of pale softening, similar to that affecting the corpus striatum, existed also to a considerable extent in the posterior lobe of the right cerebral hemisphere. The rest of the cerebral substance of this hemisphere was softer than natural, and appeared to contain less blood than ordinary. All other parts of the brain were healthy. The right middle cerebral artery just at its commencement was plugged up by a small nodule of firm, whitish, fibrinous-looking substance, which, although not adherent to the walls of the vessel, must have rendered its canal almost, if not quite, impervious. With the exception of a speck or two of yellow deposit in their coats, the rest of the vessels at the base of the brain were healthy and filled with dark blood.

The heart was enlarged; on its exterior were several broad white patches of old false membrane. The right cavities and left auricle contained recent separated coagula; the fibrin firm and whitish. The right valves were healthy; so also were the aortic, with the exception of slight increase of thickness. The mitral valve was much diseased, the auricular surface of its large cusp being beset with large warty excrescences of adherent blood-stained fibrin. There were a few scattered deposits in the coats of the aorta. The right common iliac artery, about an inch above the origin of its internal branch, was blocked up by a firm, pale, laminated coagulum, which extended into the internal iliac, and for about a quarter of an inch down the external iliac, where it terminated rather abruptly. The lower portion of the coagulum was colourless, and softer and more crumbling than the upper, which was also more blood-stained and laminated. There was no adhesion of the coagulum to the walls of the vessels. No similar clot existed in the iliac vessels on the opposite side. The pleuræ were adherent in places; the lungs œdematous, and in places solidified by compact grayish-white masses, such as might result from uncured pneumonia. The pulmonary vessels were free from old coagula.

The liver and intestinal canal were healthy. The spleen was large, pale, and soft. One large portion, about a fourth of the organ, was converted into a mass of firm, yellowish-white, cheesy substance. The kidneys were pale, rough, and granular. Within the cortex of the right were several large masses of yellow deposit, surrounded by patches of redness. The portions of medullary structure passing to these deposits were compact, dryish, and yellow.

In the case just narrated, death evidently resulted from softening of a large portion of the right side of the brain; and the cause of this softening appeared to be an imperfect supply of blood, consequent on the middle cerebral artery of the same side being obstructed by a plug of fibrin within its canal. I am not aware that there has yet been recorded a case in which fatal softening of the brain resulted from a cause like this; therefore in itself this case is one of value. That the existence of the fibrinous coagulum within the cerebral artery was the real cause of the changes in the brain, can, I think, scarcely admit of question. The sufficiency of such an obstruction to produce the effects ascribed to it is fully established by the many instances in which disturbance, or complete arrest of function in a part, with subsequent atrophy or disorganization of its tissue, results from any circumstance which materially impedes or entirely cuts off its supply of blood. Examples of this kind at once suggest themselves;—such as the weakened and subsequently degenerated heart, when the coronary vessels are diseased by deposits in their coats; the feebleness, atrophy, or even gangrene, ensuing in the whole or part of a limb whose arteries are similarly diseased, or obstructed from any other cause; also the impairment of cerebral function and subsequent softening of the tissue of the brain when the cerebral vessels are much diseased. But perhaps the best illustration bearing on the case in question is afforded by the results sometimes observed after ligature of one of the common carotid arteries. Such an operation is not unfrequently followed, almost immediately, by giddiness, loss of speech, and unconsciousness, which may pass on even to fatal coma. When not thus speedily fatal, hemiplegia of the side opposite to that of the ligatured artery may ensue, and death may occur from that cause at a more remote period, while examination after death discloses a greater or less amount of disorganization, amounting sometimes to gangrene, in the cerebral substance, especially on that side on which the operation was performed.[1]

The effects of sudden diminution in the supply of blood to one-half of the brain, are strikingly illustrated, too, by a most interesting case recently in the "Transactions" of this Society by Dr Todd,[2] in which syncope, followed by hemiplegia and softening of one side of the brain, resulted from the sudden formation of a dissecting aneurism of the aorta, innominata, and right carotid arteries, whereby the current of blood along the carotid and vertebral arteries on this side was all but completely arrested. Effects such as these, resulting from sudden diminution in the quantity of blood sent to the brain, seem to have been, to a certain extent, imitated in the case just read to the Society, in which a considerable part of one cerebral hemisphere, ceasing to receive its due supply of blood, in consequence of obstruction in one of its main arteries, at once failed in its functions, and ultimately became disorganised.

Admitting this explanation of the mischief which ensued in the brain, one is naturally led to inquire into the source of the fibrinous plug found in the middle cerebral artery. The suddenness with which the cerebral symptoms came on made it probable that the blocking up of the vessel was equally sudden, and not the result of a gradual coagulation of blood in this situation. The absence, too, of all appearance of local mischief in the coats of the vessel at the obstructed part, and of general disease of the arterial coats elsewhere, also pointed to some other than a local origin for the clot; and I formed the opinion at the time of the examination, that a portion of the fibrinous growths found on the mitral valve had become detached, and then carried with the the stream of blood up the carotid artery, until arrested at the angle whence the middle cerebral proceeds. This explanation, which fits equally well for the origin of the plug impacted in the common iliac artery, appears so reasonable, that it is difficult to doubt its correctness; for, as already remarked, it is quite conceivable that portions of the loosely-adhering masses of fibrin might be readily washed off by the stream of blood continually passing over the valve; and that when once admitted into the circulating current, such portions would necessarily be arrested the moment they arrived at a vessel too small to allow of their transit along its canal.

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[1] The whole subject of the effects produced on the cerebral circulation by ligature of the carotid artery has been so thoroughly discussed by Dr Burrows, in his work on "Disorders of the Cerebral Circulation" (pp. 72-79), that it seems quite unnecessary to add more to the remarks made on this subject in the text.

[2] Vol. xxvii.

Before commenting further on this case, or attempting to prove that the yellow masses found in the kidneys and spleen were also closely connected with the fibrinous growths on the mitral valve, I beg to offer the particulars of two other cases parallel to the last.

CASE 2.—Lousia Richards, aged 24, a thin, pale young woman, was admitted into St Bartholmew's Hospital, under Dr Burrows, in November 1851, on account of hemiplegia on the right ride, which had ensued suddenly, while at dinner, five days previously. The loss of motion was complete; that of sensation partial. Her intelligence was tolerably clear, though her articulation and memory of words were impaired. She appeared to have been in tolerable health at the time of the seizure, but had latterly been exposed to great privations. On auscultation, a loud systolic murmur was heard at the apex of the heart; the sounds clear at the base. At first the symptoms amended; but in a fortnight headache, vertigo, and increased difficulty of speech returned, and there was no steady improvement afterwards, but increasing emaciation, debility, and unconsciousness, until her death, in a state of coma, three months after admission. A few petechial spots appeared on the body a few days before death, together with swelling of the right hand and foot. Throughout its progress, the case was regarded by Dr Burrows as one of gradually-advancing softening of the left side of the brain; and the evident coexistence of extreme mitral disease invested the autopsy with unusual interest.

Examined 32 hours after death, the body was found extremely emaciated. Numerous minute petechial spots existed on the neck, chest, and extremities, and several dusky-red blotches on the ankles. The skull was thin, light, and deficient in blood. The tissue of the pia mater, over almost every part of the brain, was spotted and mottled by dark-red and pinkish blotches of extreme congestion, amounting in places almost to ecchymoses. In the midst of a few of these engorged patches were streaks of yellowish material, as if the neighbouring tissue was infiltrated with pus; but on microscopic examination the yellowish material was found to consist of multitudes of minute glistening granules, like particles of fat. Nothing like pus-corpuscles could be found; and it is probable, therefore, that the yellow material was composed merely of degenerated blood or fibrin. The surfaces of the arachnoid were smeared over by a layer of soft pinkish material, like thin mucilage. There was considerable excess of watery fluid in the cavity of the arachnoid and in the tissue of pia mater. The general substance of the brain was soft and watery, and of about ordinary vascularity. The left corpus striatum, and the portion of cerebral hemisphere immediately around it, were reduced to a soft, shreddy, almost diffluent pulp, of a pale grayish or dull white colour. The left optic thalamus appeared of ordinary consistence, as did also the corpus striatum and optic thalamus on the opposite side. The septum lucidum was entire; the fluid in the lateral ventricles pale, rather turbid. No trace of either old or recent hæmorrhagic cyst could be found. The left middle cerebral artery, immediately after its origin, was completely plugged up by a firm, whitish, oval mass, about the size and form of a grain of wheat; this mass was tightly impacted within the vessel, the canal of which it completely obliterated, while it loosely adhered to its interior. The branches immediately beyond the obstruction were reduced to firm, narrow, yellowish or rust-coloured cords. These obliterated vessels were imbedded in the pulpy, diffluent cerebral substance immediately below and in front of the softened corpus striatum already described. A similar though smaller fibrinous plug existed in the right middle cerebral artery, but did not quite block up the canal of the vessel. There was no trace of atheromatous disease of the arteries of the brain, nor of any other part of the arterial system examined. There were no old coagula in any of the cerebral sinuses, which contained recent clotted and fluid blood.

The pericardium contained a few drachms of clear fluid; there were several white patches of old lymph on the surface of the heart, and a few petechial spots. The heart was much enlarged, especially when contrasted with the general wasting of the rest of the body. The right cavities and valves were healthy; the left ventricle much hypertrophied. The mitral valve was the seat of numerous large fungous or condylomatous growths, consisting of pale, tolerably firm masses of fibrin, heaped up in warty excrescences along the auricular border of the valve, and extending for some distance along the posterior part of the interior of the auricle. The individual masses of fibrin were of various shapes and lengths, some nearly half an inch long; they were pretty firmly attached to the thick and roughened surface of the valve, yet portions could be readily detached, and, when submitted to pressure, crumbled down beneath the finger. Several of the masses extended among the tendinous cords, which were thickened and united together in bundles; one of the thickened cords was distinctly ulcerated across, while portions of fibrin adhered rather firmly to each of the separated ends.

The right external iliac artery immediately above Poupart's ligament was blocked up by a pale, firm, laminated coagulum, which was loose, yet completely filled up the canal of the vessel, the interior of which was smooth and the coats of natural thickness. The coagulum was about an inch long, beginning and terminating abruptly: externally it was quite pale, while some of the inner laminæ were blood-coloured; in one or two places the interior was hollowed into little spaces, containing a thin reddish-brown pulpy fluid. The right femoral vein was also blocked up by an almost similar coagulum, which extended along the entire course of the vessel into the ham; yet in no part was there any undue adhesion between the clot and the interior of the vein, or any other evidence of inflammation of the vessel. There were no old coagula in any other of the vessels examined; and these included the principal arteries and veins of the left lower and both upper extremities, the common and internal iliacs, the carotid and subclavian arteries, the two venæ cavæ and the main venous branches entering them; also the pulmonary arteries and veins. The blood itself was properly coagulated and separated in the cavities of the heart; yet the fibrin was scanty and pale.

In the pleuræ were a few old adhesions, and several petechial spots scattered over the surface. The lungs were healthy, with the exception of some œdema and congestion of the lower lobes. The mucous membrane of the larynx and trachea presented several petechial-looking spots. Similar spots were also scattered over the peritoneum.

The liver was pale and soft, but healthy. The spleen was much enlarged, and its texture soft; while imbedded within it were numerous various-sized circumscribed masses of solid, yellowish, cheesy-looking substance. In consistence these masses varied,—some being compact and firm, others soft, some almost pulpy. A few of the deposits were quite small, others bigger than walnuts. The existence of old coagula in the branches of the splenic artery was not clearly determined.

The kidneys, in general structure, were healthy; yet within the cortex of each were numerous yellow or buff-coloured masses similar to those in the spleen. They were of various sizes, and seemed also to vary in the length of time they had existed;—some being compact, yellow, depressed on the surface, and presenting scarcely any appearance of redness at their circumference; while others appeared of more recent date, and were surrounded by a distinct halo of spotted redness. Although existing principally in the cortex, yet streaks occasionally extended from them into the tubular structure of the gland. Examined microscopically, ordinary urinary tubules were found in the neighbourhood of the yellow masses; yet sections of the masses themselves exhibited little else than small narrow canals, apparently bloodvessels, containing multitudes of minute dark particles, similar to those found in the vessels of the pia mater, and apparently consisting of degenerated fibrin. One kidney was injected through the renal artery; the fluid passed readily into those portions of the gland which were free from the yellow masses, yet into the parts where these masses were most numerous not a trace of the injection entered.

The stomach and intestinal canal were healthy, with the exception of a few ecchymosed dots on the mucuous membrane of the stomach, and the existence of several congested patches on the peritoneal surface of the small intestine. These latter spots were distinctly caused by engorgement of the extremities of the mesenteric vessels, in which blood seemed to have been arrested some little time before death, for in the interior of the congested spots buff-coloured specks were clearly observed. No doubt the various petechial-looking spots observed in the different serous and mucous membranes, including that also of the bladder, were of the same nature, consisting of blood stagnant in minute vessels, and subsequently partially decolorised; for in the centre of almost all these spots was a distinct yellow or buff-coloured speck. The uterus and its appendages, as well as other parts not specially named, were healthy.

The principal points of resemblance between this case and the one previously related may be briefly recapitulated. These were, softening of a limited portion of the brain, producing death by hemiplegia; obliteration of the cerebral artery supplying the softened part, the obliteration being caused by an old fibrinous coagulum impacted within, but not adherent to the interior of the vessel; similar old coagula in one of the iliac arteries; fibrinous deposits in the spleen and kidneys; and the presence of large warty growths on the mitral valve.

There were other minor features of resemblance equally illustrative of the primitive cause of all this mischief; but these may be reserved until after the narration of the following case, which affords a no less striking example of the class under consideration.

Case 3.—William Purdy, aged 24, a gasfitter, of intemperate habits, was admitted into St Bartholomew's Hospital, under Dr Roupell, January 22, 1852, in a state of extreme emaciation and debility, with sloughs on his back, and hemiplegia of the left side. Both feet and legs were much swollen, while the femoral vein in each groin was found hard, cord-like, and painful on pressure. Several dusky blotches, composed of distended capillaries, were also observed on the right thigh. On auscultation a prolonged, harsh, systolic murmur was heard at the apex of the heart, fading towards the base, where the second sound was clear. It was learnt that three months previously, after exposure to cold, he was attacked with diarrhœa, to which he was subject, and severe pain across the back. He continued ill and under treatment for about two months, suffering with diarrhœa and obscure pains in his joints, which his medical attendant considered to be rheumatic. At the end of this time he was suddenly attacked with severe pain in the region of the heart, accompanied by palpitation, both of which symptoms were relieved by the application of leeches and blisters, and in a week had almost disappeared; but he still continued too ill to leave his bed. One night, about a fortnight after the commencement of the cardiac symptoms, he suddenly got out of bed, quite contrary to his usual custom, and left the room, apparently for the purpose of relieving his bowels. His wife immediately followed, and found him in a confused, bewildered state, with his left hand and arm paralysed, his face drawn to one side, and his speech impaired. Shortly after being placed in bed the left leg became powerless like the arm, and both continued so until his admission to the hospital a fortnight afterwards, at which time the loss of motion was complete, though sensation was not much impaired. He had complained of headache a few days before the seizure, but at the time of the attack he did not lose his consciousness, and he had no fit either previous or subsequent to the paralysis.

The swelling of the ankles had existed about three days previous to admission, having been preceded by pain in each thigh.

For a few days after admission he seemed to rally under the influence of tonics, nutritious diet, and wine, while the pain in the thighs was relieved by the application of leeches over the femoral veins. The amendment, however, was but temporary, and he died in ten days after admission.

An examination of the body was made twenty-eight hours after death. The emaciation was considerable. The lower limbs remained œdematous, especially the left, the foot of which was of a dark, livid colour. The tissues generally were very pale, especially about the scalp. The skull was pale and light. The membranes of the brain were healthy, but pale, while there was a considerable excess of clear fluid in the cavity of the arachnoid and the tissue of the pia mater. The vessels of the pia mater were unusually deficient in blood, almost empty. The substance of the brain was remarkably pale, soft, and watery in every part; there was no trace of a clot, or any manifest product of inflammation. Impacted within the right middle cerebral artery, just at its origin, was a firm plug of pale fibrinous substance, about the size of a hemp-seed, completely blocking up the canal of the vessel, while the branches immediately beyond the obstruction were narrow, but filled with dark stagnant-looking blood, which had quite a different character to that in the other cerebral vessels. There was no trace of any disease in the coats of the cerebral arteries, and no obstruction in the left middle cerebral. Within the left lateral sinus was a large mass of old dryish colourless fibrin, somewhat adherent to the lining membrane, which was spotted red. A piece of similar fibrin existed also in the left internal jugular, but not connected with the mass in the lateral sinus. The other cerebral sinuses and right internal jugular were free from old coagula. The pericardium was healthy within, but externally it adhered to the left pleura. The heart was about natural in size, but much diseased in its interior, the tricuspid, mitral, and aortic valves being encrusted over with large, firm, warty vegetations. On the tricuspid valve these growths were attached along the auricular surface, just above its free border. They varied considerably in size and number at different parts of the valve; many of the masses consisted of small compact roundish or oval bodies about the size of hemp-seeds, or bigger, attached singly or in clusters to the edge of the valve and to the tendinous cords to which they more or less tightly adhered. In structure they were firm and solid throughout, of a yellowish-white colour, and evidently composed of a dense fibrinous substance. The free border of the mitral valve was thickly studded with a continous ridge of rough cauliflower-like masses of firm white fibrin, which formed warty excrescences of various sizes and shapes. One mass was nearly as big as a hazel nut, firm, elastic, and solid throughout, and of a mottled yellow and red colour on section.

The aortic valves were studded by a similar crop of smaller warty vegetations. Lying loose in the cavity of the ventricle were several small brownish nodules of old bloodstained fibrin. The muscular tissue of the heart was generally healthy; but just beneath the lining membrane of the left ventricle, and occasionally deep within its substance were numerous pale yellow or buff-coloured blotches and streaks surrounded by red borders, and having the general appearance of the changes described under the term of capillary phlebitis. The coronary arteries were healthy; so also was the general arterial system, though the aorta and its main branches were very narrow. The principal venous trunks contained recent coagula, but the two external and internal iliac veins, and both femoral veins were blocked up by old variously-discoloured masses of firm, friable fibrin. These old coagula were very large, and produced great distension of the veins in which they occurred. There appeared to be no disease of the coats of the veins, and the coagula were nowhere adherent to them; and the arteries leading to the lower extremities were free from old coagula.

There was nothing peculiar in the blood generally. The coagula in the right cavities of the heart presented quite ordinary characters. There were a few scattered pleural adhesions. The lungs were generally very œdematous; both lower lobes were consolidated by masses of fibrinous deposit, consisting principally of reddish-brown pulpy material, surrounded by darker portions composed of recently extravasated or stagnant blood. In some parts of the right lower lobe were several collections of greenish thick pus, the majority being about the size of peas, a few as large as a walnut. All the branches of the pulmonary artery leading to the lower lobes, were completely plugged up by old, firm, variously-coloured laminated coagula. The branches going to the upper lobes also contained old coagula, though these were softer, apparently of more recent date, and did not so completely block up the canals of the vessels. The pulmonary veins contained recent coagula. The liver appeared healthy, though pale; and there were no old coagula in any of the portal or hepatic vessels. The spleen was enlarged to about three times its natural size, and very dark from extreme sanguineous engorgement; its tissue was soft and pulpy, while within its substance were several variously-sized circumscribed masses of fibrinous deposit. These masses varied in colour from dirty brown, spotted with red, to bright yellow; and in consistence from that of a firm, friable cheesy substance, to that of a semi-fluid pulp. One of the collections of pulpy substance was contained in a kind of cyst bounded by the capsule of the spleen; at one point the capsule had given way, and a considerable quantity of the pulpy fluid was found in the peritoneal cavity, and smeared over the intestines. The absence of any signs of peritonitis makes it probable that the escape of this material had taken place either just previous or subsequent to death. There was nothing deserving particular notice in the state of the intestinal canal. The kidneys, though healthy in general structure, were the seat of numerous, large, yellow, fibrinous masses, similar to those in the spleen. In the left, these masses were so abundant and large as almost to replace the entire structure of the gland. All the masses were firm and compact; many of them were surrounded by zones of redness. The main artery of this kidney, from the point of its entrance and along all its traceable subdivisions, was filled up by pale, firm, old fibrin. Similar, though redder, old coagula existed in the renal vein. There were no old coagula in the artery or vein of the right kidney, which, moreover, was much less diseased than the left.

In all essential respects this case closely resembles the two previously narrated. In each there was pale softening of the brain; a plug of fibrin obliterating the canal of one of the main cerebral arteries; masses of fibrinous deposit in the kidneys and spleen; and, which seemed to be the source of the mischief elsewhere, large, warty, fibrinous excrescences on the left valves of the heart.

So many, and yet such rare features of resemblance, cannot fail in demonstrating a very close connection between the several morbid appearances so exactly reproduced in each case.

Although in the autopsy of the last case it was not particularly noted that the right side of the brain, or any portion of it, was softer than the rest, yet the existence of hemiplegia on the left side, and the softening in each of the former cases being most marked at the parts supplied by the obstructed arteries, leave little room to doubt that in this case also, those portions of brain supplied by the right cerebral artery were more atrophied, though perhaps not manifestly much softer than other parts whose vessels were not thus obstructed.

Besides the existence of fibrinous vegetations on the valves of the left side of the heart, and the formation of fibrinous deposits in other parts freely supplied with blood by the left ventricle, it is worthy of particular attention that in this case there were also warty growths on the tricuspid valve, together with coagula in the pulmonary arteries, and masses of fibrinous deposit in the substance of the lungs. The importance of this fact, in support of the close and direct connection between deposits on the valves of the heart, coagula in the arteries, and fibrinous deposits in various organs, will be again noticed. At present I would allude to it merely in support of the view that the clot in the middle cerebral artery was in this, as in each of the other cases, directly derived from the vegetations on the left valves of the heart.

At first it appears singular that in each of these cases, as also in others I have had the opportunity of seeing, the clot should be found as nearly as possible in the same situation. But a glance at the arrangement of the arteries at the base of the brain, especially in an injected specimen, will make it clear that this point is, of all others, the one perhaps most likely to arrest a solid mass floating in the blood, transmitted to the brain by the internal carotid artery; for, almost directly after its entrance into the skull, the carotid divides into its two main branches, the middle and anterior cerebral, which immediately diverge in almost opposite directions. The sudden diminution in size, resulting from the division, and the different directions at once taken by the two branches, will together tend to make the angle whence the branches diverge well calculated to arrest any solid body transmitted along the carotid: while, since of the two branches, the middle cerebral is the largest, and also maintains more nearly than the anterior the original direction of the trunk from which they both sprang, a solid body seems more likely to pass into it than into the anterior division. And such is found to be the case; for, if the plug is not found sticking directly at the angle, it is found a short distance up the canal of the middle cerebral.

Once arrested at the angle, or within the canal of the middle cerebral artery, a mass of fibrine, if large enough to block up the vessel, becomes at once the cause of loss of function and subsequent atrophy to almost all that portion of the brain supplied by the obstructed vessel; for, although by the arrangement of the vessels composing the circle of Willis, ample provision is made against obstruction ensuing in any of the main arterial channels of either side *previous* to their arrival at the circle, there is comparatively little provision for an obstruction ensuing in any of the main branches into which this arterial circle breaks up. This remark applies especially to the middle cerebral artery, which, if plugged up at its origin, becomes at once almost useless as a blood-vessel; for nearly all its divisions, especially those for the central parts of the brain, proceed to their several destinations without receiving any anastomosing branch from the other divisions of the circle of Willis. The truth of this will be evident on examining an injected brain: and the fact at once explains, why the portions of brain supplied by the branches of an obstructed middle cerebral artery, are deprived of all nourishment, except the little they may receive from the minute inosculations provided by the ultimate divisions of other arterial branches of the circle of Willis.[1] The anterior cerebral artery is, by means of the anterior communicating branch, in great measure guarded against the occurrence of a similar evil; and in this way may be explained the infrequency of softening of the anterior cerebral lobes compared with the more frequent occurrence of this condition in the parts supplied by the middle cerebral artery.

I trust that the details of the three cases just submitted to the Society will be considered sufficiently satisfactory to establish the two principal points which I have been desirous of proving in this part of my communication;

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[1] Since writing the above, I find an almost similar remark made by Dr Todd, in his comments on a case already alluded to. Speaking of the fact of the softening of the brain in that case being limited to that part supplied by the middle cerebral artery, he says, "this artery is the principal branch of the carotid within the cranium, and has a less free communication with the corresponding ramifications of the opposite side than any of the other arteries of the brain. Hence the parts supplied by it are more apt to suffer than those which are nourished by the other branches of the carotid."—*Med. Chir. Trans.*, vol. xxvii., p. 321.

Namely, *first*, that softening of a portion of the brain, with attendant loss of function, may result from obstruction of a main cerebral artery by the lodgement of a plug of fibrin within its canal;

And, *secondly*, that the foreign substance thus obstructing the vessel is probably not formed there, but is derived directly from warty growths situated on the left valves of the heart.

It has long been admitted that any disease of the cerebral arteries sufficient to impede the transit of a due quantity of blood may induce softening of the brain, from imperfect nutrition. But the diseased state of the vessels to which nearly all observations on the subject apply, have reference only to the peculiar fatty or atheromatous condition so frequently presented by the coats of the cerebral arteries, especially in persons of advanced life. And I have been able to meet with very few recorded instances, in which distinct fibrinous clots have been noticed blocking up the canal of any of the arteries of the brain; and even when their existence has been noted, the conditions leading to their formation, and the relation which they bear to the attendant cerebral softening, have, so far as I know, never received an explanation similar to that which I now beg to offer.

By further research I might possibly have increased the number of instances in which old coagula have been found by different observers in cerebral arteries. Yet since this peculiar affection of the vessels of the brain seems never to have been suspected, it has never been specially sought for, and the part in which it occurs has perhaps rarely been closely examined, the mere absence of general disease of the coats of the cerebral arteries being considered as sufficient proof that the cause of cerebral softening was not dependent on any lesion of these vessels. Yet I feel convinced that had more minute attention been paid to the condition of the middle cerebral arteries, many otherwise obscure cases of white softening of the brain, especially where attended with disease of the heart, would have been cleared up by the detection of some such cause of obstruction as that in the cases I have related; and I trust that future observations will prove this to be a not unfrequent cause of cerebral softening, especially when occurring in young persons. One feels at once inclined to ascribe very many recorded cases of cerebral softening to this cause, and arguments in favour of such explanation readily suggest themselves; but, in absence of direct proof, one must, of course, remain satisfied with the mere supposition that such explanation is correct, waiting for future investigations to determine the comparative frequency of this affection of the cerebral vessels in cases of pale, non-inflammatory softening of the brain.

It is probable, too, that many cases of partial and temporary paralysis suddenly ensuing, in one or more limbs, in young persons, especially if accompanied with signs of cardiac disease, may be due to interruption of a due supply of nutriment to the brain by the temporary plugging up of a principal cerebral artery by fibrin detached from a diseased valve on the left side of the heart. Temporary loss of power in one or more limbs is not an uncommon circumstance in young persons affected with heart-disease. A good example of this class of cases was furnished by a pale, delicate girl, admitted into St Bartholomew's Hospital, in 1849, under Dr Roupell, with pain and weakness in both right limbs, numbness of the back of the right hand, and flexion of the fingers of the same extremity; these symptoms being of about a month's duration. Over the semilunar valves at the base of the heart, was plainly audible a double endocardial murmur; and this persisted all the time the girl was under notice. Many such cases of partial hemiplegia in young nervous women, are no doubt rightly attributed to hysteria; yet, on the existence of such distinct evidence of cardiac disease as that furnished by a double endocardial murmur, it would scarcely be right to deny to such affection of the heart some share in the production of the paralytic state. In what way the cardiac disease may induce the symptoms of paralysis in such cases, must perhaps always be matter of doubt; yet it seems not unreasonable to infer, that the cardiac murmur may be due to fibrinous deposit on the valves, and that portions of this deposit may have been detached, and subsequently arrested in the cerebral vessel supplying that part of the brain whence the paralysed limb derives its nervous influence.

Although the arrest of a plug of fibrin within the canal of a cerebral artery would naturally tend to impairment of function and atrophy of the portion of brain supplied by the obstructed artery, yet it is conceivable that such clot may ultimately soften, break up, and be removed, and thus the vessel become permeable again; or, as suggested to me by Dr Burrows, it may sometimes happen that the coats of the artery may, by the pressure behind, be sufficiently dilated around the clot to allow of the transit of some blood along the canal. In some such way may, perhaps, be explained the recovery sometimes observed in certain cases of partial paralysis, apparently dependent on softening, or other structural disease of the brain. But, probably, in the majority of cases, the coagulum once firmly impacted within the vessel, will tend to form an organised adhesion to the walls, and so produce permanent obliteration of the canal. In consequence of the obstruction thus produced, the blood, continualy impelled up to the obliterated part, will naturally tend to induce distension of the coats of the vessel immediately behind the seat of obstruction. And it may be a question, whether many of the aneurismal pouches found in the cerebral arteries, may not originate in this way. In favour of such a view may be mentioned the facts, that the origin of one or other middle cerebral artery is the most frequent seat of such aneurisms; that they are commonly found uncombined with any disease in the coats of the rest of the cerebral arteries; that they are not unfrequent in young persons, in whom general disease of the arterial system is rare; and that in many instances they are found associated with valvular disease of the left side of the heart.

As might be supposed, the arterial branches, at the base of the brain, are by no means the only arteries in which fibrinous masses, detached from the valves of the left side of the heart, and mingled with the circulating stream, may be arrested. In Cases 1 and 2, coagula were found in the iliac and femoral arteries, and in Case 3 in the renal; and in each of these cases the coagula were, in all probability, derived from the same source as that which furnished the clot in the middle cerebral artery, namely, the warty masses on the mitral valve. Many specimens, put up in museums, and supposed to illustrate the spontaneous coagulation of blood, or the deposition of fibrin, within limited portions of an arterial trunk, are probably to be referred to the same origin. One, in the museum of St Bartholomew's Hospital, is probably of this nature; it consists of a portion of a femoral artery, which, with the commencement of the profunda, is blocked up by a coagulum of firm, pale, laminated fibrine. There is nothing in the appearance of the coats of the vessel which make it in the least probable that they had induced coagulation of the blood, no thickening, no roughening of the interior, and no particular adhesion of the clot to the lining membrane; while it is also stated, that there was no disease in any other part of the arterial system. The source of this coagulum should, therefore, be sought for elsewhere; and I think it may be referred to the heart, for "the valves of the aorta were in part destroyed by ulceration, and there was a growth of soft, vascular fungus from their edges." It is of course easily conceivable that a portion of this soft, warty growth might have been detached, and carried to the part of the artery where it was subsequently found. The limb from which this artery was taken became, without any evident cause, pulseless and cold, some time before death.[1]

Dr Hughes[2] quotes, from Legroux, a case very parallel to this.

A woman, who had suffered with symptoms of cardiac disease, was suddenly seized with paralysis, and loss of sensation in the left arm, terminating in gangrene of the part. On her death, which occurred suddenly, eighteen days afterwards, the brachial and ulnar arteries were found plugged up by old pulpy granular fibrin; while, together with an extremely diseased mitral valve, "a concretion of exactly the same colour, texture, and consistence was discovered in the left auricle." Viewed in connection with the previous cases, the inference in this case seems natural, that the coagula in the arteries of the arm had their origin in one or more masses of fibrin, formed within and detached from the interior of the heart. The suddenness with which the paralysis of the arm ensued, is also quite calculated to favour this view, although I ought to state that a different explanation is given by Dr Hughes, who considers the clots to have originated in the parts where they were found. Cases of this kind might readily be multiplied; and I think that, for the majority of them, the source of the coagulum found in the arteries might be ascribed to portions of fibrin detached from the valves of the heart, and carried bodily to the vessels in question.[3]

As bearing closely on this as well as other parts of the general subject under consideration, I may quote a few particulars from the case of a young woman, whose heart is preserved in the museum of St Bartholomew's Hospital.[4] In the left ventricle of this heart is a large aneurismal pouch, filled by a pale, fibrinous laminated coagulum, portions of which hung into the cavity of the ventricle. For eighteen months the patient had suffered from palpitation and other signs of disordered circulation. About four months before death, she lost the use of the left arm; from this she recovered, and then the left leg became similarly affected, and was at the same time swollen and painful. This also nearly got well, and then she lost the use of the right leg; which, in turn, gradually recovered. Presently, however, the left leg again became much swollen and œdematous, and the skin affected with an erysipelatous inflammation. On the subsidence of this the patient had an attack like fever; on recovering from which there was a recurrence of paralysis in both left limbs, succeeded shortly by convulsions, of which she had several attacks, at various intervals, and ultimately died in one.

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[1] Catalogue of the Anatomical Museum of St Bartholomew's Hospital, vol. i., series 13, No. 22. In his valuable work on "Diseases of Arteries," Mr Hodgson (p. 18) mentions the particulars of a case so exactly identical with this that I cannot but think they both refer to the same subject.

[2] Guy's Hospital Reports, vol. iv., p. 164.

[3] Although in the text I have spoken of the fibrinous coagula found in the large arteries as having been probably brought thither in mass from the heart, yet I should also add, in explanation of the laminated structure they sometimes present, that the size of the mass originally detached from the heart, and arrested at an angle or recess of an artery, was probably much smaller than that of the coagulum subsequently found in the vessel, the increase in size having probably taken place gradually by the successive deposition of fresh layers of fibrin on the nucleus formed by the original mass.

The formation of laminated coagula in the corresponding veins in some of these cases is probably due in great measure, to the slowness with which the blood will move along the veins when relieved from the pressure of the arterial current, which will be so greatly diminished by the obstructing plug within the vessel.

[4] Catalogue of the Museum, vol. i., series 12, No. 53.

The body was subsequently examined by Mr Paget. The brain was found soft, with rather deficient vascularity of itself and its membranes; and there was an old cavity containing clear fluid in the right corpus striatum. The spinal cord was healthy throughout. With exception of the heart, spleen, and kidneys, all the other parts examined were healthy. Within the spleen were several circumscribed deposits of yellow ochre-coloured substance; and one of the main branches of the splenic artery was nearly filled by a firm and dark laminated coagulum adhering closely to its walls, and having internally some yellow substance like that in the spleen itself. In the cortex of each kidney were several deposits of various size, consisting of a firm yellow substance, like solid pus, surrounded by a vascular areola. Within the thin muscular wall forming the outer boundary of the aneurismal pouch in the heart, " were a number of small yellow ochre-coloured deposits, just like those found in the spleen."

The fibrinous deposits in the kidney, the spleen, and the splenic artery, were in all probability derived from portions of fibrinous clot detached from the interior of the cardiac aneurism, and transmitted with the circulating blood to those parts in which they were found. It is also not unreasonable to infer that some, at least, of the strange paralytic and phlebitic symptoms which marked the progress of the case were due to the circulation of other particles of the fibrinous substance, and their arrest in the vessels of the limbs affected with temporary paralysis and signs of phlebitis.

The hemiplegia ultimately ensuing on the left side was probably associated with an affection of the brain; but it is not unlikely that in this, as in other cases, obstruction by means of a portion of coagulum detached from the heart had ensued in the right middle cerebral artery, and induced a state of softening, of which the cavity found in the right corpus striatum was the remains.

II. Having considered some of the principal circumstances apparently connected with the detachment of comparatively large masses of fibrin from the interior of the left side of the heart, I proceed, in the second place, to offer a few observations on some of the effects which smaller portions, similarly detached and arrested in minute arterial branches or in capillaries, appear capable of producing. One of these effects seems to be displayed by the singular masses of yellow fibrinous-looking substance not uncommonly found in the spleen, kidneys, and other organs, and hitherto described under such names as "capillary phlebitis," "metastases," or "fibrinous deposits."

In the details of the three cases already narrated to the Society, several varieties of these deposits have been rather fully described; and it seems unnecessary, therefore, to offer any further description of them here. They are familiar to all who have seen much of the inspection of bodies after death, and their several peculiarities have furnished subjects of former communication to this Society by Dr Hodgkin and others. My present object is to shew that these morbid appearances are very commonly associated with valvular disease of the heart, especially with those forms of disease attended with the deposition of fibrinous vegetations on the valves; and that in the majority of cases, if not in all, they result from the direct transmission of particles of fibrin from the valves of the heart or elsewhere, and their subsequent arrest in the vessels of the parts in which these morbid deposits are found.

Out of 21 cases in which I have observed these deposits in the spleen, kidneys, or other parts supplied with blood directly from the left side of the heart, and in which I have noted the condition of the heart and other principal organs, I have found disease of the valves or of the interior of the left side of the heart in every instance but two; and of these two exceptional cases, one was a case of cholera, in which a doubtful mass of capillary phlebitis existed in the liver, the other a case of aneurism of the aorta,—which, as I shall afterwards shew, tends rather to prove than disprove the explanation I offer of the fibrinous masses existing in the various organs. Omitting these two cases, however, I find that of the remaining 19 it is not merely stated that the heart or the valves were diseased in each of them, but that in 14 there were fibrinous growths on the surface of the left valves or interior of the left cavities, while in the remaining 5 there is simple mention of valvular disease, without any statement as to whether fibrinous deposits existed or not.[1] The mere fact of so large a number of cases of so-called capillary phlebitis in internal organs being distinctly associated with the presence of fibrinous material on the valves of the heart, is sufficient to suggest a very close relation between these two morbid states; and the existence of some such close relation is rendered more probable by the absence, in all these cases, of any other condition likely to have induced a poisoned state of the blood, to the existence of which the formation of these deposits has not unfrequently been ascribed. In many of the cases cardiac dropsy is noted as the fatal disease, while in none of them does there seem to have been any proof of the existence of a so-called blood-disease, such as typhus, purpura, or the like.

That such blood-diseases have, indeed, nothing to do with the deposits in question seems proved by the fact that, out of the examination of a large number of fatal cases of fever, I have never yet met with an instance in which masses of capillary phlebitis existed in any part of the body in this disease. From their general absence in other diseases, then, as well as from their frequent occurrence in diseases of the heart, we have equally strong reason for believing in the existence of a close connection between these morbid deposits in internal organs and the presence of fibrinous growths on the inner surface of the heart or on its valves.

The close connection subsisting between endocarditis and fibrinous deposits in distant organs has, I am aware, been strongly insisted upon by Rokitansky, and has also attracted the notice of Hasse; therefore it may seem superfluous to have said so much on the subject. But I would plead a twofold excuse for so doing: first, because, so far as I know, the connection pointed out by Rokitansky has never yet been confirmed, scarcely even recognised, in this country; and secondly, because I believe, with all deference, that it may admit of an interpretation somewhat different from that which Rokitansky has given. After careful and repeated perusal of all I can find bearing on this subject in Rokitansky's great work on Pathological Anatomy, I cannot find that he in any place even hints at the explanation I have ventured to offer of the real cause of the secondary deposits in distant organs in cases of valvular disease of the heart. His observations seem to shew quite plainly that he ascribes them to a poisoned state of the blood, consequent on the admixture of the products of endocarditis with this fluid, whereby it acquires an increased tendency to coagulate, which tendency cannot well be manifested in the arteries, owing to the velocity of the current, but is brought about within the capillaries on account of the slowness with which the blood there moves, and the increased facility which is thus afforded for the morbid material derived from the inflamed heart to exercise its influence on the composition of the blood.[1]

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[1] For similar confirmatory cases see Dr Jackson (Med. Chir. Transactions, vol. xxix., p. 280), and Dr Ormerod (Gulstonian Lectures, Med. Gaz., 1851).

The view, however, which I have ventured to take is, that the deposits in the various organs are the direct mechanical results of the arrest of solid particles of fibrin detached from some part of the heart or arteries, and too large to traverse the minute capillary canals to which they are brought by the circulating blood. By the obstruction which their arrest occasions, they may induce coagulation of blood behind them; while, by their mere presence, they may act as local irritants, and so induce secondary processes of inflammation and suppuration, like any other foreign body. In either or both of these ways may be produced the various appearances characteristic of these singular deposits. This view,—which seems supported by the other evidence I have adduced of the direct transmission of masses of fibrin from the valves of the heart,—has the advantage, too, of explaining some of those cases in which very similar fibrinous deposits are found in various organs, independent of any warty or other growths within the heart. For example, in a fatal case of aneurism of the abdominal aorta, to which I have already alluded, several patches of capillary phlebitis existed in one of the kidneys, though for the origin of these there seemed to be no other explanation than that afforded by the existence of fibrin abundantly deposited in laminated masses within the sac of the aortic aneurism. It is easy to conceive that portions of such fibrin might be broken up, mingled with the circulating blood, and subsequently arrested within the capillaries of the kidney or spleen.

An example even more marked than this was afforded by the case of a man admitted, under Dr Burrows, in a dying state, in whom, after death, there was found a small aneurismal pouch, about the size of a Spanish nut, immediately behind one of the aortic valves. Its interior was lined by old laminated fibrin; and the aortic valves, which were themselves greatly diseased, being thick, hard, and rigid, had their roughened edges and surfaces covered with recent fibrinous fringes. The spleen was enormously enlarged, being upwards of 9 inches long and 6 inches broad; the increase in size was principally due to several large masses of firm, yellow, cheesy-looking substance, one being as big as a good-sized apple. The kidneys were large and firm, and presented on their surface numerous spots of a deep red colour, in several of which was a yellowish central dot. These changes were manifestly the result of a similar morbid process to that in the spleen, only in an earlier stage.

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[1] Handbuch der Pathol. Anat., Bd. i., s. 242, e. s. Bd. ii., s. 437.

In this, as in the last instance, the mere mechanical detachment of fibrin from an intra-vascular part whereon it was deposited, seemed to be the true explanation of the masses of fibrinous substance found in the spleen and kidneys, for in no other way could their origin be reasonably accounted for.

In some few instances, in which masses of capillary phlebitis are found in internal organs, the artery supplying the affected part may be found plugged up by old dry colourless fibrin, as was observed in the renal artery in Case 3. When this occurs, it may be a question whether the deposits in the organ do not result from the sudden arrest of a plug of fibrin in the artery, and the consequent coagulation and subsequent change of the blood in the vessels beyond the seat of obstruction. But although this may be the explanation in some instances, yet since the coagula found in the large arteries can usually be traced onwards into their various subdivisions, it seems reasonable in such cases to ascribe the coagula in the various arterial tubes to gradual stagnation of the blood in them, consequent on the more minute vessels being successively blocked up by the entrance and arrest of fresh fragments of fibrin, which, though small enough to pass through the arteries, may be too large to traverse the capillaries.

Although, however, there may thus be a doubt in some cases, whether the primary obstruction took place in an artery or in the capillaries, yet in other instances it appears to be quite manifest that the fragments of fibrin have been arrested in the minutest vessels of the affected organ. The ordinary masses of capillary phlebitis are often of large size; but if the tissue of an organ, such as the kidney, in which these large masses are found, be closely examined, the surface will usually be seen presenting reddish spots or blotches of various size and shape, like patches of extreme congestion, or even small ecchymoses, while towards their centres these patches frequently exhibit a pale yellowish or buff-coloured appearance, as if the stagnant blood of which they consisted was becoming gradually decolorised at these parts. There can be little doubt that these patches are in their nature identical with, though smaller, and in an earlier stage of transformation than the large yellow masses with red zones in other parts of the gland. Together with these distinct patches, one may also often find in the same organ, spots of such extreme minuteness as to be scarcely visible without a lens: they are mere red dots, like minute petechiæ, yet in the centre of almost every one may be discerned a distinct yellowish or fawn-coloured speck, shewing that in nothing but their extreme minuteness do they differ from the larger blotches of capillary phlebitis.

I would direct especial attention to the fact of the co-existence of these minute spots with the large and clearly-discernible masses; for since it seems to prove the identity of the two forms of disease, it likewise makes it probable that the small red spots with their yellowish centres, even when found uncombined with the larger masses, have originated in the same cause which led to their formation in other cases. By thus regarding them as mere modifications of ordinary capillary phlebitis, we seem to obtain an explanation of several otherwise unintelligible morbid appearances, and at the same time advance another step towards the elucidation of various pathological phenomena. Thus in Case 2 it was mentioned, that besides the coagula in the cerebral and iliac arteries, and the masses of distinct capillary phlebitis in various internal organs, minute red petechial-looking spots were found in many parts of the body, namely, the pericardium, pleuræ, and peritoneum, and the mucous surface of the larynx, trachea, stomach, and urinary bladder, and elsewhere; also that nearly all these red spots presented a pale yellowish or buff-coloured centre. The characters exhibited by these spots was so exactly like, on a small scale, those presented by the ordinary patches of capillary phlebitis, with which indeed they co-existed in some of the organs, that their identity was considered unquestionable. Associated, too, as they were with large friable masses of fibrin on the valves of the left side of the heart, it seems scarcely to admit of doubt that they, as well as the larger patches of capillary phlebitis, were caused by the coagulation and gradual decolorisation of blood rendered stagnant by the arrest of minute particles of fibrin separated from the deposits on the valves and circulating with the blood.

This view of their origin is strongly supported by the results of the direct introduction of minute particles of any material into the blood; for in nearly all such cases, similar small congested or echymosed spots, with more or less of a yellowish centre, have been found after death in the lungs and other parts. The results of some of M. Gaspard's experiments are especially to the point. Thus, in one quoted by Mr Lee,[1] half an ounce of fluid, resulting from the decomposition of some beef placed in dog's blood, was injected into the jugular vein of a bitch. After death, the lungs were found gorged with blood, and they "presented many petechial spots, like small ecchymoses." Similar spots existed also on the left ventricle of the heart, in the spleen, mesenteric glands, gall-bladder, mucous membrane of the intestines, and sub-cutaneous cellular tissue. This, and other analogous experiments, as well as the appearances after ordinary phlebitis, strongly favour the opinion that the ecchymosed spots, sometimes found so abundantly in various parts of the body, may result from the arrest of fragments of fibrin separated from warty excrescences on the valves of the heart and circulating in the blood. The numerous parts in which these spots are found in some cases, clearly indicate that the blood has been highly charged with some such material; and one cannot wonder at the serious and even fatal results which not unfrequently attend such cases, particularly when various organs, including the brain, simultaneously become the seats of these congested spots. The symptoms in such cases are sometimes most obscure, apparently because of the vitiated state of the blood, which produces constitutional effects very similar to those observed in continued fever or phlebitis.

III. This brings me to the consideration of the third series of effects which I stated at the commencement of this paper, to result sometimes from the introduction of fibrinous particles into the circulating blood;—namely, the manifestation of phenomena like those indicative of the existence of a morbid poison in the blood. The following case, selected from several similar to it, may be narrated as affording a good illustration of this part of the subject.

Case 4.—Richard Griffith, aged 14, a healthy-looking boy, but who had been lately badly off and much stinted for food, was admitted into St Bartholomew's Hospital under Dr Roupell, Feb. 12th, 1852, with obscure typhoid symptoms, and a petechial eruption on the skin. He was conscious, but gave an imperfect account of himself, and it was only after his death that a clear history of the case was obtained. It was to the effect that he had been in good health until a fortnight before admission, when he began to complain of pain in the right groin; this pain continued without material abatement and without particular affection elsewhere, for about ten days, and then he was suddenly seized with shivering, headache, and pains in the back and calves of the legs, followed by heat, thirst, and general illness. On the onset of these symptoms he was brought to the Hospital and prescribed for,—his condition not being one of such urgency as to call for admission. He continued to attend for two days, and then, at the request of his mother, was taken in, and was put on a plan of treatment suited to an attack of fever, under which he was supposed to be labouring. He was confused and rather delirious the first night, but was quite rational the following morning, and said he felt ill and thirsty. The second night he was more quiet; and on the ensuing morning, shortly after taking a dose of his saline medicine, he went to sleep, but from this sleep he passed into a half comatose state, in which he continued until his death, about twenty-four hours afterwards.

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[1] On Phlebitis and Purulent Deposits, 1850, p. 34.

The body was examined about thirty hours after death.

It was rather emaciated, while over every part of the surface were numerous petechial spots, some recent and of a pinkish colour; others apparently older and of a dusky red hue; the majority of the spots were small, but a few were as big as split peas. The rigidity was universal and very marked.

The skull was natural; the dura mater healthy. Almost every part of the tissue of the pia mater, including the folds between the convolutions, was infiltrated with what seemed to be recently extravasated blood, which gave a blotchy dark red appearance to the surface of the brain. In the midst of these red blotches were several yellow or buff-coloured spots and patches of various size; and even when the dark red spots were very small, the central part had a similar light yellow colour; some of the patches had a greenish-yellow appearance, as if smeared over with pus. The brain itself was unduly congested; the bloodvessels on section being numerous, and singularly large and turgid with dark blood; there were a few small spots like ecchymoses near the surface. The cerebral arteries and sinuses were healthy.

The pericardium was healthy, except in the existence of several petechial spots on the surface of the heart; the heart itself was of natural size; the right valves healthy. The auricular surface of the mitral valve, just above its free border, presented a ridge or fringe of whitish fibrinous vegetations, which adhered very slightly to the thickened surface of the valve, and when scraped off were found remarkably soft and friable. Large warty masses of similar soft whitish vegetations adhered to the ventricular sides of the "surfaces of contact" of the aortic valves; these deposits were chiefly arranged in little heaps along the lower festooned border of each cusp, but in places they extended over almost the entire surface of the valve. The central portions of each of the contiguous halves of two cusps were destroyed by ulceration; parts of the ulcerated tissue hanging loose and covered by flakes of fibrin. The general tissue of the heart was healthy, yet the substance of each ventricle, especially on the inner surface, was marked by numerous scattered petechial spots, in the centre of almost every one of which was a yellow dot.

The lungs were generally healthy; there was some congestion of the lower lobes, with puriform mucus in the smaller bronchial tubes. The pleuræ were healthy, excepting several petechial spots on their surface.

The spleen was very large, dark and soft; there were several yellow masses of fibrinous deposit near its surface.

The kidneys were healthy in texture; yet on stripping off the capsule the surface of each was found studded with minute petechial spots, in the centre of almost every one of which was a small buff-coloured dot; besides these dark red spots there were several large yellowish blotches extending deeply into the substance of the cortex, and surrounded by a reddish halo; between these large masses and the minute petechial dots, were several other stages evidently connecting together these two extreme forms of one and the same morbid condition. No old coagula were found in any of the renal vessels.

The intestinal canal was healthy, except that along almost its whole extent the mucous membrane was spotted with petechial and larger ecchymosed patches. Similar petechial spots existed in the mucous membrane of the bladder, of the pharynx, œsophagus, stomach, larynx, and trachea; also on the folds of the peritonæum. Some of the spots from the latter were examined carefully with the naked eye as well as microscopically; and it was observed that the minute vessels about them were gorged with blood, while within several of these distended vessels, solid coarse lumps, apparently of fibrinous matter, were clearly discerned with the microscope. The liver, and other parts not specially named, presented nothing worthy of note.

This was a case full of obscurity from the commencement, and the obscurity increased as the symptoms of cerebral oppression came on, and proved so rapidly fatal. The lad's depressed languid aspect, with an abundant petechial eruption, seemed to indicate, in the absence of a clear history, the existence of low fever; and the restless, delirious manner in which he passed his first night in the Hospital, favoured this view, and justified the employment of wine and simple salines; yet the fatal coma which rapidly supervened two days after admission, was quite unintelligible on this supposition, and could only be understood by an examination after death.

Then the whole mystery seemed to be cleared up; and the most reasonable interpretation which can be offered of the phenomena observed during life and the appearances presented after death seems to be this. The pain in the right groin with which the attack set in was rheumatic; then ensued rheumatic inflammation of the mitral and aortic valves, with ulceration of the latter, and deposition of fibrinous vegetations on both. From these fibrinous deposits, many of which were loose and easily detached after death, portions had probably separated during life, and, being mingled with the blood, were transmitted with it to all parts of the body; then, arrested in the capillary networks and smaller arteries, they produced the various petechial-looking spots in the skin and most of the serous and mucous surfaces of the body, the buff-coloured blotches and streaks, and the solid fibrinous masses in the kidneys and spleen, while they probably caused the fatal issue of the case by the pressure resulting from the extreme engorgement of the minute vessels of the pia mater and the substance of the brain.

Viewed by itself alone, this case is of extreme interest; but taken in conjunction with others it seems to possess features of great pathological value, as illustrating the serious effects which may result from direct poisoning of the blood, by the products of rheumatic inflammation of the valves of the heart being mingled with it. In the typhoid symptoms noticed during life, and the secondary deposits found after death, the case presents features almost exactly parallel with those ensuing in phlebitis after wounds. In both cases, indeed, the exciting cause of the morbid phenomena seems to be nearly the same, namely, the introduction of some morbid material into the blood, and its subsequent transmission with that fluid to various parts of the body. It sometimes happens, too, that in the progress of rheumatic disease of the valves of the heart, the similarity to phlebitis is made more striking by the formation of abscesses in various parts of the body, and the deposition of pus within the joints.

A good example of this seems to be furnished by the case of a young woman admitted under Dr Burrows in February 1851, suffering from rheumatic affection of several large joints, of three or four weeks' duration. The articular pains ultimately settled in one shoulder, which joint continued to suffer until the patient's death, about two months subsequently. From the period of admission the case assumed an unfavourable aspect, the subsidence of the rheumatism being attended by no improvement in her general condition. In about three weeks, after occasional rigors and a state of almost constant profuse perspiration, a painful swelling formed behind the right angle of the lower jaw, and seemed likely to suppurate; but it disappeared. Then both lower limbs gradually became œdematous, with tenderness in the popliteal space and in the groin. The œdema increased, the patient became more exhausted and hectic, and so died.

After death a large abscess was found in the right axilla. Yellowish turbid fluid existed in the left shoulder-joint, the cartilage of the head of the humerus being somewhat ulcerated. The inferior caval vein, as well as both common iliacs and their branches, were blocked up by old decolourised fibrin, the left external iliac vein containing, besides old fibrin, about a teaspoonful of yellowish puriform fluid. There was an old clot in one of the large branches of the pulmonary artery; also several buff-coloured spots of capillary phlebitis on the kidney, and extreme disease of the aortic and mitral valves, one cusp of the aortic and a large patch of the mitral being destroyed by ulceration; while shaggy flakes of soft fibrin loosely adhered to the irregular borders of the ulcers, and to other parts of the valves.

Without commenting on any of the pathological features of this case, I offer it as an illustration of the occasional coexistence of the signs and effects of phlebitis with rheumatic ulceration of the valves of the heart; and I would suggest that in this, as in the previous case, the vitiated state of the blood leading to the secondary deposits in various parts of the body, was the direct result of the introduction of morbid material from the inflamed and ulcerated valves of the heart, over which the blood would be continually flowing, and thus almost necessarily washing away the solid matter deposited on the diseased surfaces of the valves. It seems, indeed, quite obvious that the blood may be contaminated in this way almost as certainly as when fibrin, or pus, or any other organic or inorganic material in a state of fine division, is directly introduced into it by experiment or otherwise. The kind of constitutional effects produced may in some measure be determined by the nature of the foreign material mingled with the blood; yet the mechanical effects will probably in each case be nearly the same; for whether the foreign particles introduced be those of fibrin or of quicksilver, or anything else, if they are too large to traverse the first set of capillaries they reach, they will be arrested there, produce obstruction, and possibly lead to circumscribed inflammation and suppuration.

Materials introduced into the blood by experiment, absorption, or otherwise, commonly enter the venous blood, and with it are transferred to the lungs, when they exercise their primary and, perhaps, their only effects. But when the blood is contaminated by materials derived from the valves of the heart, it is arterial blood which is especially affected, and all the systematic organs and tissues are liable to suffer, because of the far greater frequency with which the left valve, compared with the right, are diseased. One wonders, indeed, why well-marked symptoms of contaminated blood do not more commonly accompany deposits on the left valves of the heart. It may be that such symptoms only arise when the morbid matter mingled with the blood is derived from the soft, semifluid, perhaps almost putrid, material resulting from the decay of old-standing masses of fibrin, while recently deposited granules, washed away and mixed with the blood, may merely cause symptoms of irritation in the parts through which the blood circulates, or, at the same time, produce the fibrinous masses of so-called capillary phlebitis in various organs. It seems not improbable that many anomalous symptoms ensuing in the course of certain diseases, acute rheumatism for example, in which there exists a great tendency to the deposition of fibrin on the valves of the heart, may have their explanation in the irritation or other effects resulting from the existence of minute fragments of fibrin in the blood circulating through the organ whose functions are disordered. I would suggest that many functional disorders of the nervous system, especially chorea, may be thus explained. The frequent existence of a cardiac murmur in chorea, and the presence of warty vegetations on the valves of the heart so commonly found in fatal cases of this disease, are in favour of such a view.

PART II.

On the effects which may result from the detachment of Fibrinous Deposits from the right valves of the Heart.

If, from what has been stated, it be assumed as probable that deposits of fibrin occurring on the valves of the left side of the heart may, by being detached, be productive of serious affections of remote organs, it may be inferred also that similar deposits occurring on the right valves may induce corresponding secondary affections of the lungs. And there seems to be sufficient evidence for believing that such is really the case. For it may, I think, be clearly shewn, that most of the fibrinous or other similar secondary deposits in the lungs, also many of the old coagula found in the pulmonary artery or its branches, and possibly some forms of pulmonary apoplexy, are closely connected with, if not actually dependent upon, fibrinous deposits on the valves, or interior of the right side of the heart, or materials transmitted through the heart, by venous blood. It is of course conceivable that when the deposit on the right valves consist of large warty masses, as they occasionally do, portions of considerable size may be detached, and transmitted along the pulmonary artery, and so plug up one of the large branches of this vessel, just as similar masses detached from the left valves may be arrested in one of the main systematic arteries; but I have not yet met with a decided instance of such an occurrence.

Probably the more usual manner in which the separation of fibrinous masses from the right valves leads to the formation of coagula in the pulmonary artery, is by the transmission of small particles to the minuter divisions of the artery, or to the capillary plexus, at which they induce stagnation of the blood in those branches of the artery distributed to the seats of obstruction. Such a result is almost necessarily consequent on the peculiar mode of distribution of the branches of the pulmonary artery, which pass to their destination without anastomosis. In a paper on the formation of coagula in the pulmonary artery, published in the Transactions of this Society, Mr Paget has clearly shewn the influence which certain obstructions in the pulmonary capillaries, such as œdema, chronic pneumonia, and pulmonary apoplexy, sometimes exercise in inducing coagulation of blood in the arteries supplying the obstructed parts. And I have likewise noticed a similar influence apparently resulting from other circumstances, such as extensive old tubercular disease, and extreme compression of the lung by false membrane on the pleura, which have obliterated large portions of the pulmonary tissue. Mr Paget also narrates instances in which particles of cancerous matter brought from remote organs to the right side of the heart, and thence transmitted to the lungs, became arrested in the pulmonary capillaries, and so induced stagnation and subsequent changes of the blood, in branches of the pulmonary artery. Cases like these, of which I have seen several examples, seem to leave no doubt, that a like coagulation of blood in the pulmonary arteries may result from obstruction caused by the arrest of particles of fibrin detached from the right valves of the heart, and transmitted to the pulmonary capillaries. An instance of this is furnished by one of the cases already narrated (Case 3), in which, together with large nodular and warty masses attached to the tricuspid valve, nearly every branch of both divisions of the pulmonary artery were blocked up by old fibrinous coagula.

Another equally striking illustration is afforded by a specimen in the Museum of St Bartholomew's Hospital, in which, with extreme disease of the pulmonary valves, accompanied with the deposition of thick irregular layers of soft fibrine on each of them, there were old coagula filling many of the branches of the pulmonary artery. In this case there were also several large, solid, fibrinous masses in the substance of the lung; and it seems reasonable to believe that these, as well as the coagula in the pulmonary artery, had their origin in the deposits of fibrin on the pulmonary valves, portions of which were probably detached, arrested in the capillary plexus of the lungs, and so caused the fibrinous masses in the pulmonary tissue, and the consequent coagulation of blood in the arterial branches distributed to these parts.

The fibrinous masses in the lungs which the specimen just mentioned presents, appear not unlike portions of old pulmonary apoplexy, from which most of the colouring matter of the extravasated blood has been removed; and it is not improbable that many similar masses in other cases may have originated in a like cause, and not in hæmorrhage into the pulmonary tissue. Such masses, indeed, represent one form of the appearances described as capillary phlebitis of the lungs, or, in other words, one stage in the transformation undergone by blood stagnant and coagulated in the pulmonary capillaries. This blood passes through the same changes in the lungs that it undergoes when similarly situated in other organs; and the various examples of these changes are not unfrequently met with in the lungs. Thus, in Case 3 were found various gradations, from firm compact coagula, through soft, brownish, disorganized blood, to collections of yellowish, puriform material, which in places formed ordinary abscesses. Masses of such large size, and with such obvious characters of these, are of course readily recognised. Yet not unfrequently deposits of a similar nature exist in the lungs, though of such extreme minuteness as to elude detection, unless specially sought for. These consist of small, slightly elevated, red dots, with a pale yellow or buff-coloured centre, scattered, sometimes thickly, over the surface and within the interior of the lung. They are exactly identical with that spotted form of capillary phlebitis already mentioned as often occurring in systemic organs and in various tissues, either combined with other forms or alone. When met with in the lungs, I have hitherto invariably found it either as the result of some morbid material in the venous blood, or in direct connection with affection of the right valves of the heart; such affection, namely, as is attended with the deposition of fibrinous granules on the surface of the valves. To quote but one instance out of several of the kind, I would mention the case of a girl under the care of Dr Hue, early in the year 1851. This patient died suddenly, after suffering for some months with symptoms of extreme disease of the heart. Besides general enlargement of the heart, and narrowing of the mitral orifice, the free border of the tricuspid valve was studded with small, pale, fibrinous granules, a few of which existed also on the pulmonary valves. At first sight the lungs appeared healthy, but, on closer inspection, they were found freckled throughout with small, dark red spots, like minute ecchymoses, in the interior of several of which was a distinct, buff-coloured speck. The view which may not unreasonably be taken of these spots is, that they consisted of congested capillaries, in which minute fragments of fibrine, transmitted from the right valves of the heart had been arrested, the appearances, indeed, being just such as resulted from the injection of softened meat into the blood in one of M. Gaspard's experiments.

Under whatever form these various deposits are met with in the lungs, I believe that careful examination will shew them to be almost invariably associated either with the presence of fibrinous growths on the right valves of the heart, or with some other condition leading to the existence of particles of fibrin or other foreign matter in the blood transmitted to the lungs. Of these other conditions the most important seem to be the disintegration of old masses of fibrin situated within the right cavities of the heart, and a like disintegration of old coagula in some part of the venous system, and its subsequent mixture with the venous blood. It appears to be quite usual for the old colourless or pale reddish clots found in the right cavities of the heart, especially in the appendix of the auricle, to soften in the centre, and be converted into a dirty reddish-brown or fawn-coloured material. Sometimes the softening extends through the whole substance of the mass, with the exception of a thin layer at the circumference, which forms a kind of cyst or bag within which the softened material is contained. Sometimes too this cyst bursts and discharges its contents, leaving nothing but the outer shell attached to the interior of the heart. The softened material thus let loose and mingled with the blood will doubtless contaminate it almost as effectually as the direct introduction of a similar material by injection into a vein would do. And it is easy to imagine that the solid particles of fibrin may be arrested at the capillaries of the lungs, and produce the various forms, especially perhaps the spotted variety of deposit to which allusion has been made. Old coagula in the veins too, under whatever circumstances they may have originated, appear almost equally liable to undergo softening, and to break up and mingle their disintegrated particles with the venous current along which they may pass to the lungs, and produce effects similar to those consequent on the transmission of like material from the cavities of the heart.

Such are some of the principal effects which the transference of fragments of fibrin from the right side of the heart appear capable of producing in the lungs. Much more might be said on the subject; but the length to which this communication has already extended precludes any further remarks at the present time. I would only add the suggestion that possibly the peculiar form of the pneumonia sometimes observed in rheumatic fever may, in some way, have its explanation in the transmission of fibrinous particles from the right valves of the heart to the lungs. The almost invariable existence of disease of the pulmonary or tricuspid valves in the fatal cases of rheumatic pneumonia I have examined after death strongly favours the opinion that there is some close relation between this peculiar inflammation of the lungs and the fibrinous deposits on the right valves of the heart.

In conclusion, let me briefly recapitulate the principal points I have endeavoured to establish to the satisfaction of the Society. They are, 1*st*, the general fact that fibrinous concretions on the valves or the interior of the heart admit of being readily detached during life, and mingled with the circulating blood: 2*dly*, that if detached and transmitted in large masses, they may suddenly block up a large artery, and so cut off the supply of blood to an important part; if in smaller masses, they may be arrested in vessels of much less size, and give rise to various morbid appearances in internal organs; while, under other circumstances, the particles mingled with the blood may be extremely minute, possibly the debris of softened fibrin, yet in sufficient quantity and with sufficient power to produce a poisoned state of the circulating fluid, as manifested in the production of typhoid or phlebitic symptoms: 3*dly*, that the effects produced and the organs affected will be in a great measure determined by the side of the heart from which the fibrinous masses have been detached; for, if the right valves have furnished the source of the fibrin, the lungs will bear the brunt of the secondary mischief, displaying it in coagula in the pulmonary arteries, and various forms of deposit in the pulmonary tissue: but if, as is far more commonly the case, the left valves are affected, the mischief is more widely spread, and may fall on any systemic part, but especially on those organs which, such as the brain, spleen, and kidneys, are largely and directly supplied with blood from the left side of the heart.

ART. X.—*An Account of some Experiments and Observations made on the Body of an Executed Criminal.* By Dr ALBERT KOLLIKER, Professor of Anatomy and Physiology in the University of Wurzburg. (Communicated by the Author.) [From Annals of Anatomy and Physiology. Conducted by JOHN GOODSIR, F.R.SS.L. & E., &c., No. II., Edinburgh 1852, p. 107.]

In Wurzburg, on the 2d November 1850, at forty-five minutes past nine o'clock, the murderer, Henry Schuhman, suffered decapitation with the sword.

Opportunities of this kind becoming more and more rare, Professor Virchow and I determined to avail ourselves of it, to make observations on the body immediately after death; but especially to institute some experiments on the irritability of the smooth muscular fibre, and researches on the internal structures of the eye.

Notwithstanding our own exertions, and the obliging assistance of the Magistrates, we could not procure the body before twenty minutes past ten o'clock;—thirty-five minutes after death, on account of the distance of the place of execution from the Anatomical Institution,—the only locality of which we could avail ourselves. We nevertheless succeeded, during the first hour, in obtaining results, most of which are of importance.

The following statement contains a full account of our experiments, already communicated to the Physico-Medical Society of Wurzburg; and we have only, in addition, to remark, that they were performed with the kind assistance of our colleague Professor Rinekir, and in the presence of many teachers and students of the University, and of Professor Gerlach of Erlangen, who happened to be here at the time.

The temperature of the apartment in which the observations were made, was 11° R. (56¾° F.) The body was very muscular; and its temperature thirty-five minutes after death, in the abdominal cavity, was 31° R. (101¾° F.); in the right cavity of the chest 29½° R. (98⅜° F.)

1. *Central Organs of the Nervous System.*—At thirty-five minutes after death these organs had already lost all irritability. By the application of both poles of a very powerful magneto-electrical apparatus to the lower surface of the section (of the spinal cord), not the slightest result was obtained. On the other hand, by direct application to the muscles, one pole being applied to the spinal marrow, and the other to any part of the trunk or extremities, strong contractions were excited, even to raising of the arms, shortening of the thorax, &c.

2. *Nerves.*—In forty-five minutes after death, no effect was perceived on irritating the roots of the oculo-motor nerves; nor any convulsion or movement of the manducatory muscles on irritating the roots of the trigemini.

In one hour and thirty-five minutes after death, the trunk of the crural nerve could not be excited; but contractions of the sartorius and rectus femoris could be produced by irritation of the branches of the nerve (insulated on slips of glass) distributed to these muscles; which continued to act in this manner for ten minutes afterwards.

3. *The Muscles* of the trunk and extremities, as well as those of the head, exhibited the most lively contractions when the body was first brought in for observation.

In one hour and five minutes after death, the irritability was as yet very considerable; in half-an-hour later it was already less; and in two hours and five minutes after death, at the time when the experiments were concluded, it was very weak, but still perceptible. It is scarcely necessary to mention, that by means of the induction apparatus, contractions were produced in all the muscles, which ceased immediately on breaking the circle. When the irritability began to decline, there also occurred contractions of a clonic character, and principally in the cremaster, in which they were very distinct, and several thickened contracted parts were perceived on this muscle even after the poles were removed.

4. *The Heart* did not contract when we opened the chest, forty-five minutes after death; although the temperature within the pericardium was, as has been already stated, above 30° R.

The coronary veins contained air. When we galvanized the apex of the right auricle, it contracted very slowly; and some time after, the whole auricle began regularly to contract, which, however, ceased on the circle being broken.

The contractions of the auricle having been once excited, it was only necessary afterwards to apply the pole for the shortest time to its tip, to produce complete contraction. Rhythmic motions, also, similar to the normal, could be obtained by successive applications, and removals of the pole. Irritation of the left auricle and ventricle gave no result; but it is necessary to observe that both ventricles, and particularly the left, were found in a very contracted condition.

5. *Spleen.*—We directed our attention particularly to the spleen; having been induced to do so, more especially by the recent communications of Harless.[1] It was the first organ we laid hold of. The splenic vessels were tied, and the spleen having been cut out and insulated, it was irritated by one pole of the apparatus, armed with a metallic plate, one inch in diameter, being laid upon it; while a needle connected with the other, was inserted not far from the plate (a method by which very complete contraction in the spleen of the dog may be produced); but in vain, although we moistened the spot on which we wished to produce contractions. We were equally unsuccessful after irritation of three other spots on the two surfaces of the organ. These results coincided with my most recent researches, from which it appears that the human spleen does not possess contractile elements.[2]

The human spleen has hitherto been only twice galvanized, by Harless,[3] and by ourselves; but apparently with entirely different results. Harless thought he perceived contractions, while we could see none. Some of our negative results may perhaps be considered of no great importance in opposition to those of Harless. But it must be recollected, in the first place, that the body on which we experimented was under observation twenty minutes sooner than the one on which Harless made his researches; in the second place, that the spleen which we galvanized was by no means contracted, but was rather to be considered large, and felt soft, although its surface presented slight inequalities; and, in the third place, that almost all the smooth muscles of the other parts of the body still reacted in a very lively manner.

It must also be added, that the experiments of Harless afford very insignificant results, and even that his interpretation of these may be questioned. Thus Harless observed, after inserting two needles, half-an-inch from each other, near the hilus of the organ "the substance between them rise in the form of a small mound, which was slowly formed, and, after interruption of the circle, again slowly disappeared." From this he concludes that the human spleen is contractile; the rather as the mound in question would not form again, after the experiment had been repeated for the third and fourth time.

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[1] Jenaische Annalen. 1850.

[2] Art. Spleen.—Cyclop. of Anat. and Phys.

[3] *Loc. cit.*

I must confess, however, that this conclusion appears to me very rash. Because, in the first place, it does not appear to me likely, that a contraction of the spleen would produce the rising of a mound-like elevation; and, *secondly*, because I hold it to be very improbable, that a part rendered prominent by contraction under galvanic influence would sink into a state of relaxation *immediately* after the circle is interrupted.

In reference to the first point, it is to be remarked, that with the known anatomical conditions of the spleen, no conception can be formed of a rampart-like elevation produced by contraction of the trabecular texture, or of the capsule, in which alone the muscular fibres could have been situated. In fact, too, neither R. Wagner[1] nor I[2] observed any such elevations on the spleens of animals (dogs) in our successful experiments; but we distinctly observed a wrinkling and hardening of the surface, and the formation of ribbon-like and circular firm spots, phenomena easily explained by contraction of the trabeculae attached to the capsule, or of the latter itself.

It appears to me, too, that the rapid subsidence of the rampart-like elevation which Harless saw, is opposed to its having been formed by the contractions of organic fibres; because it is almost the general law of such fibres to continue for a longer or shorter period in a state of contraction before they again relax. There are certainly organic muscular fibres which soon pass from a state of contraction to that of relaxation, after interruption of the galvanic irritation, as in the iris in man, and some of the mammalia; but in all other instances, the directly opposite result takes place. It is known that in the lower animals, very prolonged contractions of the stomach, intestines, bladder, bloodvessels, and ducts of the glands occur; and in regard to the spleen in particular, R. Wagner and I have found this to be the case. The firm wrinkled spots on the surface of the organ remained for a long time after the poles of the battery were removed. The same peculiarity exists in man, as I have noticed it in the bloodvessels and lymphatics; and we have had again in the present instance an opportunity of observing it in the bladder, vasa deferentia, ureters, œsophagus, and scrotum.

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[1] R. Wagner. Götting. Anzeig. Nachrichten von der Universt., &c. Anzeig, 1849. St. 92.

[2] A. Kölliker. Mittheilung. der Zurich. Natursforsch. Gesellschaft. 1850. No. 41. St. 49.

It is therefore more than probable, that had the Harlessian elevations on the human spleen been produced by organic muscular fibres (of the vessels, or of some other part), they would by no means have disappeared so soon, and hence I am inclined to express my opinion, that they must be accounted for on some other ground; the more so when I consider that, according to my investigations at least,[1] the human spleen does not contain organic muscular fibres.

The spleen having been cut through, beautiful Malpighian bodies made their appearance in the greatest abundance. They were nearly half the size of those in the spleens of the ruminants, arranged very close together, and frequently gathered into groups. They exhibited nothing peculiar, except that their cells were half as large again as the lymph globules in the thoracic duct. The light red pulp and the splenic vesicles shewed no trace of a metamorphosis of the blood-corpuscles.

6. *The Skin*, under galvanism, rewarded us with very excellent results. Fifty-five minutes after death, one pole of the apparatus was applied to the mons pubis the other to the scrotum. A minute afterwards, the latter, which had previously been quite relaxed, had already begun to wrinkle, and in one minute and a half there were formed strong closely arranged transverse wrinkles, with weaker perpendicular folds, which were as well expressed as those usually formed during life, and remained also for a long time after the removal of the poles.

The Areolæ of the Mammæ were then irritated, both of them with the most perfect success. A cutis anserina had begun to form in twenty to thirty seconds on the margin of each areola; and the latter contracted, with apparent elevation of the nipple, as vigorously as it ever does during life, continuing contracted for five minutes, beyond which we did not continue to observe them.

The Skin of the Fore-Arm, and immediately afterwards *of the Thigh*, was irritated one hour and twelve minutes after death. On both a completely marked but *quite local* cutis anserina arose, on a circular area of about one inch diameter, and there followed erection of the hairs, in this individual, pretty well developed. A cutis anserina also arose on a portion of the skin, which was irritated, after having been cut off from the thigh.

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[1] Kölliker. Art. Spleen.—Todd's Cyclop. of Anat. and Phys.

7. *The Iris.*—Very remarkable motions of the iris were produced by the galvanic irritation. When one pole was applied forty minutes after death to the lower jaw, and the other to the cornea, the pupil contracted simultaneously with the muscles of the face, uniformly and pretty quickly, and quickly dilated again after removal of the poles. These results were always obtained on repeating the experiments on both eyes.

The poles (needles) were now placed on the margins of the cornea, or on the surrounding sclerotic. The contraction did not occur; but the pupil was irregularly dilated.

Several additional experiments shewed, that when the poles were applied to the upper and under margins of the cornea, the pupil became *longitudinally oval;* on applying them to the right and left margins, it became *transversely oval,* and consequently that a partial contraction of the radiating contractile structures took place. These latter motions also began pretty quickly, and ceased again as quickly.

8. *The Bloodvessels.*—Several experiments with galvanic irritation were instituted on the bloodvessels. At fifty minutes after death a branch of the superior mesenteric vein contracted, but not so much as to obliterate its cavity. Immediately after, the same effect was produced on a beautifully injected lymphatic vessel of the aortic plexus, but to a greater extent. One hour after death, the thoracic duct was galvanized immediately above the diaphragm. It appeared to contract a little, but on account of its deep situation, the effect could not be decidedly observed. We saw, however, very decided contractions in a lymphatic vessel of the lumbar plexus. One hour and five minutes after death, the abdominal aorta and vena cava, the latter much distended with air, and the common iliac artery, were irritated without any effect; but immediately afterwards, the great saphena vein, and the lymphatic vessels of the groin, exhibited most beautiful contractions, with obliteration of their cavities, and continuing long after the irritation had been removed. After one hour and ten minutes, the saphena continued as irritable as before; on the other hand, the crural vein contracted feebly, and the femoral artery also in a minor degree, although still distinctly. The trunk of the vena portae, irritated one hour and fifty-two minutes after death, afforded no result.

9. *Excretory Ducts of Glands.*—Several of these were irritated. Irritation of the gall-bladder fifty minutes after death yielded nothing which could be safely relied upon. On the other hand, immediately afterwards, the left ureter, having been only touched, contracted with great energy, so that a wave appeared to run down the tube, which became shorter

and narrower; in which condition it afterwards remained for a long time. The liveliness of the movement made upon us such an impression, that although we had several other observations yet to make, we immediately seized upon the right ureter, and with it we obtained the same distinct results.

Thirty-five minutes after death the bladder contracted very considerably, but slowly, and remained so. One hour and thirty minutes after death, the right vas deferens was exposed in the pelvis up to the inguinal canal, and irritated. It contracted and shortened itself slowly, but very considerably, so as to become quite straight, although previously it had been very much curved in its course. It even raised itself up, and became stretched (prall) and tense. On seeing this, we were involuntarily reminded of the conceptions which Physiology forms to herself of the mode in which the Fallopian tubes apply themselves to the ovaries; and we were obliged to admit, that if they can act with as much energy as the vasa deferentia, the process was quite intelligible. The other vas deferens contracted also principally in its length, but not so energetically, and continued like the left, shortened and narrowed for a long time afterwards.

10. Immediately afterwards, as an experiment, we inserted a needle into the corpus cavernosum of the penis; but even after galvanising for two minutes, we could not perceive any shortening of the organ. About ten minutes afterwards, however, on having our attention again directed to this region, the penis appeared to be really smaller, and its skin wrinkled, although we could not satisfy ourselves decidedly regarding it. One hour and fifty minutes after death, we still perceived contractions in the epididymis, and also on the lower part of the tunica vaginalis propria, at the part of its external surface on which I have described a layer of organic muscular fibres. It is to be regretted that we forgot the prostate and vesiculæ seminales, which certainly would have presented phenomena of contraction. The stomach and intestines, which we irritated fifty-eight minutes after death, did not contract; but the lower part of the œsophagus contracted slowly, and continued so for a long time.

In addition to these experiments, the anatomical and microscopical constitution of some of the organs was studied. The following is a short abstract of the results.

11. *Retina.*—Immediately after receiving the body, we examined on the retina of the left eye the macula lutea and the plica centralis. The eye had been cut through transversely immediately behind the lens, and the part of the retina remaining, the posterior portion of the organ submitted to examination. The plica centralis was missed; but the macula

lutea was present, and within it was a darker point, like a small roundish pit, the so-called foramen centrale. The retina itself had a transparent greyish aspect. The pigment of the choroid shining through, and the yellow spot had still more of a brownish-yellow colour, with a light yellow border, and without any sharp definition from the surrounding retina. A piece of the retina, taken from the situation of the macula lutea, and laid on a plate of glass, appeared of an *intense citron tint.* This tint, under the microscope, was seen to arise from a homogeneous light yellow infiltration of the parts; so that all the elements of the retina,—cells, granules (Körnchen,) and bacilli, appeared to be well preserved. The retina of the right eye, which was examined one hour and five minutes after death, also wanted the central fold, but presented a perfectly distinct yellow spot and central foramen. A further examination was deemed unnecessary, the more so as the eye was to be preserved in chromic acid for subsequent researches on the elements of the retina.

12. *Ventricles of the Brain.*—Lastly, the ventricles of the brain were examined to ascertain the existence of ciliary movements, but in vain. The cells of the ventricular epithelium were most distinctly seen, but no trace of cilia could be detected.

ART. XI.—*Observations on the Meteorology of Canada.* By WILLIAM WINDER, M.D., Librarian to the Honourable the Legislative Assembly of Canada.

In the Edinburgh Medical and Surgical Journal for April 1853, in my paper on the above subject, the tables referred to were accidentally omitted, I now beg to subjoin them.

Medium Temperature of the Air in Lower and Upper Canada.

	1847. Mean of the Month.		1848. Mean of the Month.		1849. Mean of the Month.	
	Montreal.	Toronto.	Montreal.	Toronto.	Montreal.	Toronto.
January,	Unknown	Unknown	18·90	27·41	11·70	18·49
February,	13·25	22·48	19·00	26·28	Unknown	Unknown
March,	23·55	26·25	27·60	27·40	31·66	33·24
April,	39·36	39·00	42·80	40·67	39·60	38·74
May,	61·45	53·49	61·00	53·74	54·20	48·30
June,	70·00	58·14	70·00	62·54	71·50	63·00
July,	77·76	67·62	73·50	65·37	75·70	67·82
August,	72·00	63·93	72·80	68·34	74·00	65·00
September,	59·50	54·91	57·40	53·39	59·10	57·00
October,	45·00	43·71	45·00	46·38	46·30	44·94
November,	35·60	38·00	31·40	33·61	41·00	41·87
December,	29·90	30·00	23·45	29·12	17·90	26·56
Total Means,	527·37	497·33	543·05	534·25	522·66	504·90

	Mean of the year.
Montreal 1847, .	47·94
„ 1848, .	45·25
„ 1849, .	47·51
Total,	140·70
Mean of three years,	46·90

	Mean of the year.
Toronto 1847, .	45·21
„ 1848, .	44·52
„ 1849, .	45·90
Total,	135·63
Mean of three years,	45·21

Mean of Montreal,	46·90
Mean of Toronto,	45·21
Total,	92·11
Mean average temperature of Lower and Upper Canada,	46·05° F.

Mean of Montreal,	46·90
Mean of Toronto,	45·21
Difference of temperature between Lower and Upper Canada for three years,	1·69° F.

The observations at Montreal are from those published in Dr Hall's Montreal Medical Journal (the *Brit. American Journ. of Med. and Physical Science*), and the observations at Toronto are from those made at H. M. Magnetical Observatory, under the superintendence of Capt. Lefroy, R.A.

From Dr Stratton's paper on the Meteorology of Prince Edward Island, (*Ed. Med. and Surg. Journ.*, April 1853), it appears that the thermometric mean at Charlottetown for the year 1851 was 41·7, being 5·20 lower than Montreal, and 3·51 lower than Toronto.

PART II.

CRITICAL ANALYSIS.

ART. I.—*A Practical Treatise on Inflammation of the Uterus, its Cervix and Appendages, and on its Connexion with Uterine Disease.* By JAMES HENRY BENNET, M.D., Member of the Royal College of Physicians; late Physician-Accoucheur to the Western General Dispensary; formerly House Physician *by concours* to the Hospitals St Louis, Notre Dame de la Pitie, and La Salpetriere, Paris, &c. Third Edition. London, 1853. 8vo, pp. 532.

2. *On Diseases of Women and Ovarian Inflammation, in Relation to Morbid Menstruation, Sterility, Pelvic Tumours, and Affections of the Womb.* By EDWARD JOHN TILT, M.D., Senior Physician to the Farringdon General Dispensary, and to the Paddington Free Dispensary for the Diseases of Women and Children; Fellow of the Royal Medical and Chirurgical Society, &c. Second Edition. London, 1853. 8vo, pp. 276.

Of the first edition of the present work which appeared in June 1845, an account in some detail was given in July 1846 in the sixty-sixth volume of this Journal. The principal object of that notice was to make known the peculiar doctrines of the author regarding the pathological causes of Leucorrhœa and other Uterine and Vaginal Diseases attended by puriform, purulent, or muco-purulent discharges. As the doctrines of Dr Bennet were in some respects new, and different from those to which professional persons had been previously accustomed, we rather avoided giving any opinion as to the degree in which they were correct or otherwise. But as Dr Bennet had formed his opinions and deduced his inferences from a very extensive field of observation, and one in which the cases were both more numerous and of a different, and more varied, if not of a severer character, than those usually witnessed in the cities and towns of Great Britain, it seemed right to place these doctrines as distinctly and faithfully before the profession as possible, in order that they might be subjected to that degree and form of examination, which might shew them to be founded on a small or a large collection of facts, and to represent the regular and standard, or only the exceptional instances of utero-vaginal disease.

The Treatise in its first form unquestionably awakened interest and attracted attention; for in no long time the author found it requisite to publish in 1849[1] a second edition. This he informed his readers was in many respects a new work. It contained much more detailed information on the nature and causes of the diseases brought under notice; it presented also more ample and if possible more forcible and conclusive evidence, that his doctrines were in general well-founded, and that his representations as to the prevalence of inflammatory disorders of the utero-vaginal textures were not over-strained; and it enforced still more decidedly than in the first edition the indispensable necessity in all cases of uterine and utero-vaginal discharge, of sterility, of abortion, of amenorrhœa and dysmenorrhœa,—of ascertaining by physical examination the exact condition of the *os uteri*, the *cervix*, the vagina, and other parts, which might be the seat of morbid action.

Meanwhile an auxiliary to the aid and support of the doctrines of Dr Bennet appeared in the person of Mr Whitehead of Manchester, whose work was published in 1847, and was very fully examined in the sixty-ninth volume of this Journal in January 1848. We then took occasion to advert to the important confirmation of the general correctness of the doctrines taught by Dr Bennet, which was afforded by the experience of Mr Whitehead. It is a leading feature of the work of Mr Whitehead that it contains, though collected from different sources and in an independent manner, very full and strong evidence of the justness of the leading principles advanced by Dr Bennet.

Various circumstances concurred to prevent us from giving of the second edition of the work of Dr Bennet such an account as it seemed to deserve. As, however, the third edition is now placed before the profession, it becomes requisite to direct the attention of practitioners, both medical and surgical, to the consideration of a subject upon which it is of the utmost importance that they should entertain correct ideas, both pathological and therapeutic.

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[1] In the present Edition, the date of the Second Edition is given 26th March 1845. But this is evidently a mistake for 1849.

The condition called inflammation, especially chronic, is an important element in many diseases, either as an adjunct, an antecedent, or a consequent; and in the morbid states of the womb and its appendages, the part which it performs, Dr Bennet undertakes to shew, is one of great moment, whether we look to the uniformity with which it is present, to the effects which it produces upon the uterus and its functions, or to the means by which the physician may expect to restore the organ to its healthy state, and re-establish the health of the patient. Inflammation, or a state analogous to it, and not to be distinguished from it, and occasionally its natural effects, are, according to Dr Bennet, the cause not only of AMENORRHŒA, DYSMENORRHŒA, LEUCORRHŒA, and various forms of utero-vaginal discharge, but of Sterility, Abortion, and all those conditions of the uterus and ovaries which render the former organ incapable of its function of sustaining a living fœtus during the usual period of pregnancy. Inflammation also, generally chronic, constitutes an important part of all those forms of alteration in position which the uterus is liable to undergo; and it is an effect or adjunct not uncommon of many of the new growths to which the organ is liable.

The inflammatory state, however, which gives rise to all these evils is not ordinary inflammation affecting the entire organ. Though this may be the case, it is however greatly more common for this morbid action to affect the *Cervix,* or Neck and Orifice of the Uterus, and especially the Mucous Membrane by which this part is lined, and the Muciparous Follicles with which this membrane is provided. In other circumstances it affects the whole mucous membrane of the uterus.

Dr Bennet begins the discussion on Inflammation of the Uterus in a peculiar manner. He first undertakes to explain the circumstances which render inflammation of the uterine *cervix,* a frequent instead of a rare disease. That is, he presumes first that it is a frequently prevalent affection; and then he assigns reasons why it should be so. These circumstances are the following.

In the *first* place, while the body of the uterus presents a very dense and non-vascular structure in the unimpregnated state, and contains cellular tissue in an elementary form only; the uterine cervix is of a less dense structure, is more vascular, indeed all but erectile, round the *os*, and has, distinct from the cavity of the body of the uterus, a cavity, the mucous membrane of which is occupied by numerous mucous follicles.

In the *second* place, the uterus, when not impregnated, is the seat of a periodical secretion and excretion of a certain quantity of blood from its interior, this excretion commencing with a peculiar action in the ovary, terminating in the exclusion of a mature ovum. The interpretation of this action, and its attendant phenomena, according to Dr Bennet, is that during one-third or one-fourth of each month, the uterus of a menstruating female, and especially the *cervix*, the most vital region, are physiologically in that condition, which, throughout the economy, is the immediate antecedent of inflammation, namely, a state of congestion. When on the other hand it is considered, argues Dr Bennet, that the arrest or suspension of a secretion from a congested organ, is one of the most frequent causes of inflammation, and by how many causes the menstrual flux is liable to be arrested or modified, it need not be matter of surprise, that inflammation should occur in the Uterus and its Neck, apart from physical lesions, but rather a source of astonishment, that it should not occur more frequently than it actually does.

This is very nearly equivalent to saying that if we consider the structure, the delicate organization of many organs, and the rough usage to which they are occasionally, nay often, subjected, it ought to be astonishing that inflammation does not more frequently attack every one of them; the Brain, the Lungs, above all, the Stomach, the Liver, the Spleen, the Pancreas, the Kidneys, the Heart. The truth seems to be, that congestion or inflammation, especially chronic, takes place in the textures composing the Uterus; and this is all that can be said to be known about it. The fact is certain; but as to its causes, it seems very questionable whether anything is accurately known.

If the mere organization of the Uterus, and the organism taking place in its vascular system every three weeks, were really the causes of inflammation attacking the organ, the prevalence of morbid states of the Uterus would be much greater than it is; and scarcely one female could be expected to be free from some of the forms and degrees of these inflammatory uterine conditions. It is not improbable that the author was aware of the weakness of these arguments; for he is obliged immediately afterwards to admit that "with some females the uterus is naturally a weak organ," (p. 34). This admission, indeed, is another mode of expressing the facts already stated; for the admission of natural weakness in a certain proportion of either the sex, does away entirely with the force of the argument deduced from anatomical structure and functional processes, or it is a different mode of saying that the structure is feeble, imperfect, and unfit for the actions, and that the actions instead of taking place in a normal, easy, and natural way, are irregular, performed with difficulty, and attended with imperfect results. This state of natural weakness, again, in the organ, Dr Bennet allows, is indicated by all the usual symptoms which have been remarked as taking place in females in whom the menstrual function is disordered and suppressed or perverted. In other words, the author finds it requisite to admit the evidence of this natural weakness, in order to account for the occurrence of all the disorders incident in a certain proportion of females to the Uterus and its appendages.

Inflammation of the body of the Uterus, Dr Bennet allows to be a rare disease. Not so inflammation of the *Cervix Utero*, which he represents to be an exceedingly common disorder (p. 34); so common indeed, he adds, that the great majority of females who apply at public institutions for relief, while labouring under uterine symptoms, physical or functional, will be found on careful examination to be suffering from its operation. LEUCORRHŒA, DYSMENORRHŒA, MENORRHAGIA, IRRITABLE UTERUS, PROLAPSUS UTERI, and similar affections, which are supposed to be independent of inflammation, are in reality in nineteen among twenty cases, where confirmed, the immediate result of inflammatory action in the *Cervix*, and consequently can in no other mode be efficiently treated, except by removing the primary disease on which they depend. Leucorrhœa, especially when chronic, and persisting during the entire interval of menstruation, is in almost all instances the result of chronic inflammation and ulceration of the Uterine *Cervix*. Most of the intractable cases of Dysmenorrhœa, Amenorrhœa, and Menorrhagia, are the result of local inflammation. Irritable Uterus is, adds Dr Bennet, only another name in most instances for Uterine Inflammation.

These doctrines have been becoming for at least eight or nine years familiar to most practitioners, who have practical acquaintance with the disorders of females. They contain a certain proportion of truth; but there are points in which they require limitation. Of these limiting points we have had occasion to speak formerly, both in noticing the Treatise of Dr Tilt, and that of Dr Ashwell. Irritable Uterus is, there is good reason to believe, if not always an inflammatory disorder, certainly to be treated much more effectually and easily by antiphlogistic measures than by others. Dysmenorrhœa and Amenorrhœa are probably in many circumstances attended by inflammation and congestion; but in a certain proportion of cases, inflammation properly so called, there is reason to believe, is not a part of the disorder. It is in these circumstances that the skill of the experienced physician becomes apparent. To treat all upon general principles, and according to general rules as if all were inflammatory, is certainly neither rational nor safe. But to accommodate the treatment to the circumstances of each case, is a method much more likely to be attended with successful results. This is the duty of the skilful and judicious physician.

Hysteria aud Chlorosis, the author properly exempts from the list of diseases now specified. The former has been long allowed to be a disease of the nervous system; and Dr Bennet does not propose to remove it from this position. Chlorosis, as a disease of the blood and sanguiferous system at large, is best treated by means of agents which tend to amend and improve the state of the blood and the functions of the circulating agents.

After these preliminary explanations, Dr Bennet proceeds to treat of inflammation as it affects the Uterus in whole or in part, and its appendages.

Metritis, that is, inflammation of the uterus (*Metra*), is acute or chronic, and may affect the uterus either in the unimpregnated or in the impregnated state. Acute *Metritis* of the unimpregnated undeveloped uterus is allowed to be a rare disease. This the author thinks is the natural result of the peculiar dense, fibro-muscular, non-cellular structure of the body of the Uterus. As tissues of this kind are but slightly susceptible of inflammation, in consequence of their structure, if the uterus be exposed to the causes of inflammation, this action falls generally and principally upon the periphery of the organ, the cervix, the mucous surfaces, or the lateral ligaments, all of which possess much higher vital properties than the body of the organ

On the other hand, when the state of the uterus is modified by the extraordinary development in organization and vital properties which take place during pregnancy, and during the enlargement of a fibrous tumour, a different state of matters is recognized. If the uterine tissues are then subjected to the action of the causes of inflammation, that action is ready to attack the body of the organ, and *Metritis* is then observed to take place with great intensity and virulence. These differences depend upon the great differences which exist between the uterus in its unimpregnated and its impregnated condition, when its organization is as it were increased, and its vitality exalted.

Acute *Metritis* may affect either the whole of the uterus, or part only. When it affects the unimpregnated uterus, it rarely extends to the peritoneal tissue. According to the author, it affects, with this exception, the entire organ, including the *cervix*. It is, however, an affection so rare, that it is unnecessary to dwell upon it. The author allows that he has not seen the Uterus of a female who had died of this disease in the non-puerperal state.

Chronic METRITIS is a much more common disorder. It differs from the acute in this respect, that it is more frequently partial than general, and usually occupies a limited extent only of the uterine substance. In this its partial form, it affects, in nine instances among ten, the posterior wall of the uterus in its inferior region, immediately adjoining the base of the *cervix*. The preference which Chronic *Metritis* thus evinces for this particular region is, the author suggests, probably to be accounted for by the band of longitudinal muscular fibres which pass into the posterior region of the *cervix* from the posterior wall of the body of the womb; chronic *metritis* being generally the result of chronic inflammation of the *cervix*, extending to the uterus. The author allows, nevertheless, that the disease may exist in the anterior wall or in the lateral region of the uterus.

The most important symptoms are those ascertained to be present by physical examination.

"On examining the womb digitally, in addition to the evidence of coexisting disease of the cervix which is usually detected, if the inflammation is general, the entire uterus is found enlarged, and sensitive on pressure. When it is partial only, the finger passed carefully behind, before, and on the sides of the uterus, carrying the cul de sac of the vagina before it, so as to explore its walls, readily discovers the seat of the disease. Instead of the finger passing from the base of the uterine neck on to a smooth, insensible surface, a continuation of the plane formed by the cervix, it meets with an exceedingly sensitive elevation or protuberance, sometimes quite regular, sometimes irregular and knotty. In the latter case, however, the nodosities that diversify the tumefied surface are all perfectly spherical; there are no knife-back ridges or sharp irregularities. Pressure on these tumefied surfaces is exceedingly painful. Occasionally there is no perceptible tumefaction, but merely an exquisite sensitiveness in a limited region of the uterus; pressure giving rise to the sensation of sickness. The womb is, in most instances, quite moveable, but the attempt to move it is attended with great pain.

"The uterus is not bound down and fixed in a certain position, like the liver or the kidneys. In order, no doubt, that it may be able to enlarge during pregnancy, it is loosely suspended in the pelvic cavity, and kept in its normal position as much by the contractility of the vagina and the pressure of the surrounding organs, as by its ligaments. As a necessary consequence, the partial tumefaction of the walls of the uterus that follows chronic inflammation is invariably attended with greater or less displacement of the body of the organ, the nature of the displacement varying according to the seat of the tumefaction. If the posterior wall is the seat of inflammation and enlargement, as is generally the case, the additional weight in this region causes the body of the uterus to fall backwards, towards the cavity of the sacrum. The uterus, in a word, is retroverted, and the cervix is generally anteverted, that is, directed upwards, towards the pubis; the finger having to be passed deeply into the pelvic cavity, towards the sacrum, to find the root of the cervix and the tumefied posterior uterine wall, which is lying on the rectum.

"In the form of uterine retroversion that occurs during pregnancy, the anteverted cervix approximates more and more to the pubis as pregnancy advances, until it presses on the urethra, and impedes the flow of urine. This is not observed in retroversion from inflammation, the increase in volume of the body of the uterus being comparatively slight. Moreover, in the latter form of retroversion, the cervix often remains in its usual position, and is not anteverted, notwithstanding the displacement of the uterus. In that case, it forms an angle with the body of the uterus, which is said to be retroflexed.

"When it is the anterior uterine wall that is inflamed and tumefied, the uterus may fall forwards, especially in married females, and there is anteversion of the body of the organ, which, instead of gravitating backwards into the sacral cavity, falls forwards towards the pubis, the cervix being retroverted. If this is the case, the anterior vaginal parietes are often so stretched by the extreme retroversion of the cervix, that it is difficult to examine digitally through it the anteverted uterus, so as to ascertain satisfactorily the presence of tumefaction and enlargement. This, however, may be accomplished with care and attention, the bladder being previously emptied, or at least the existence of a limited painful region may be ascertained, which, coupled with the displacement and the other symptoms, is conclusive as to the existence of chronic inflammation and enlargement."—(Pp. 47–49.)

Such is the state of the physical symptoms, or the state of the organ itself.

There is, besides, a train or assemblage of general symptoms which must not be neglected, and can seldom be overlooked. These are found in the aspect, countenance, and physiognomy of the patient, in the pain which she suffers both in the pelvic and uterine regions, and in the back, in the wasting of person and loss of strength, in the feeling of squeamishness, and other gastric symptoms, and not unusually in the presence of headache more or less severe, disordered vision and hearing, want of sleep, and visitations of disagreeable dreams.

The disease ought to be distinguished from Uterine Irritation, from Displacement of the Uterus, Retroversion or Retroflexion of the Uterus, and from Functional Dysmenorrhœa, and, above all, from Cancer affecting the Uterus.

When the uterus of a female who labours under chronic *Metritis* is examined after death, the inflamed region of the uterus is found enlarged, thickened, and more loaded with blood than in the healthy condition. If the chronic inflammation is terminated by Induration, the texture of the diseased part is more than usually dense, and of a grayish or grayish-red colour.

Another form of uterine inflammation is Internal Metritis, or inflammation of the uterine inner or mucous membrane. This the author describes in the following manner.

" *Seat.*—By internal metritis, or uterine catarrh, is meant inflammation of the mucous membrane lining the cavity of the uterus. The very existence of this mucous membrane was formerly called into question, but it is now universally admitted and described by anatomists, although its peculiar organization, as we have seen, renders its anatomical demonstration difficult.

"Much stress has been laid of late years on uterine catarrh by continental writers, and it has been described by some, not only as a very common disease, but also as the cause of most of the inflammatory and ulcerative affections of the cervix met with in practice. In reality, however, such is not the case. Internal metritis is a *rare* form of uterine inflammation, and has only been considered common because it has been confounded with inflammation of the *cavity of the cervix*, a disease, which, on the contrary, is very often met with.

" The frequency of inflammation in the cavity of the cervix, and its general limitation to that cavity, are dependent, in a great measure, on anatomical conditions, which we will briefly recal. The mucous membrane that lines the cavity of the cervix, is more vascular than that of the uterine cavity, and presents a greater number of mucous follicles; many of which are concealed between the rugæ of the arbor vitæ, the mucous membrane accurately following the depressions and commissures of the latter. The cavity of the healthy cervix is also distinctly separated from that of the body of the uterus by a constriction or natural sphincter, which has not been described by anatomists, but which is, generally speaking, sufficiently powerful to offer a decided obstacle to the introduction of the uterine sound into the cavity of the uterus, in the healthy state. The cavity of the cervix is often deeper by half an inch, than that of the uterus itself. The uterine sound, when passed into the uterus, is concealed to the extent of two inches and a half; of which one inch and a quarter, or one inch and a half, occupies the cavity of the cervix. In the latter case one inch only is in the uterus.

"The latest continental writers on uterine catarrh have fallen into the error of describing inflammation of the cervical canal as uterine catarrh. Whenever, on examining the cervix with the speculum, muco-pus is observed issuing from the os uteri, they conclude, without further examination, that it proceeds from the *cavity* of the uterus, and that the latter is the seat of inflammation. They do not reflect that the muco-pus *may* proceed, as it really does in nineteen cases out of twenty, from the cavity *of the cervix*. The result of a careful scrutiny of all the cases of inflammation of the cervix uteri that I have seen for many years, with reference to this point, has shewn me that in the immense majority the inflammation does not extend into the cavity of the uterus. I have been led to this conclusion by the observation of the following facts:—Firstly, The dilatation which invariably *accompanies* inflammation of the cavity of the cervix does not, generally speaking, extend beyond the internal constricted point, or 'os internum,' the latter remaining contracted, so as not to allow the free admission of the sound into the uterine cavity. Secondly, Therapeutical means carried so far only as the morbid dilatation exists, or to the os internum, effectually cure the inflammation, and put a stop to the discharge.

"In some few cases, on the contrary, the os internum participates in the relaxation of the cervical cavity, so that the sound passes freely into the uterus, the two cavities communicating. When this is noticed, the cavity of the uterus may or may not be inflamed; if it is, the discharge from the os uteri is more abundant, and presents peculiar characters, the local and general symptoms are rather different, and what is conclusive, therapeutical agents carried into the cavity of the cervix alone may not be sufficient to effect a cure. These latter cases are really cases of internal metritis, or uterine catarrh. The former, by far the more numerous, I look upon as cases of inflammation of *the mucous membrane and follicles of the cavity of the cervix only, or of cervical catarrh.*

"*Causes.*—All the causes which give rise to acute or chronic metritis may also occasion internal metritis. It appears, however, to be generally met with in practice as the result of the lengthened existence of inflammatory disease of the cervix and of its cavity. The inflammation gradually progresses along the cavity of the cervix until it reaches the os internum, and passes into the uterus. Indeed, considering the extreme frequency of inflammation of the entire cavity of the cervix, it is only surprising that the disease should so generally stop at the internal sphincter of that organ.

"Among the causes most likely to give rise to internal metritis, a prominent position must be given to the inflammation that occurs after parturition and abortion. When inflammation of the uterus follows the expulsion of the ovum, the surface on which the placenta was implanted is peculiarly liable to be attacked, and the seeds of chronic inflammation of the uterine lining membrane may thus be sown. In some exceptional cases, blennorrhagic inflammation may be a cause of internal metritis, the inflammation gradually extending from the vagina to the cervix, to its cavity, and to that of the uterus. This, however, I believe to be much less frequently the case than has been asserted.

"*Symptoms.*—Internal metritis being nearly always complicated by inflammation of the cervix, of its cavity, or of the substance of the womb, its symptoms are rather difficult to unravel; so difficult, indeed, that I do not believe the task has yet been accomplished satisfactorily by any writer. Internal metritis may be said to exist to a certainty, if the os internum of the cervix is so completely open as to allow the uterine sound to pass freely into the uterine cavity: if that cavity is increased in size, and more sensitive, and if, likewise, there is a more or less abundant *sero-sanguinolent* discharge, accompanied by dull, deep-seated pain in the region of the uterus itself—that is, behind and slightly above the pubis,—and by a certain amount of general febrile reaction.

The sero-sanguinolent discharge is the most important of these symptoms; indeed, it may be said to be as characteristic of internal metritis as the rust-coloured expectoration is of pneumonia. The presence of blood in the secretion from the inflamed mucous surfaces is in both cases owing to the same cause—viz., the extreme tenuity of the ciliated epithelium. The blood-corpuscles exude in inflammation, as in pneumonia or inflammation of the air-cells of the lungs, and blood is expelled mingled with the secretion of the inflamed surface. This sanguinolent discharge, however, is not always present when there is inflammation of the interior of the uterus. It is only when the inflammation is severe, or in its period of greatest intensity, that it is to be observed. At the onset, in the period of decrease, and sometimes throughout the entire duration of the disease, the secretion may be merely muciform or puriform. When congestion alone remains, it may consist only of transparent mucus. If this is the case, it becomes more difficult to distinguish internal metritis from inflammation of the cavity of the cervix, in which the same discharges are present; in both, they may be seen issuing in a thick stream from the os uteri, when the cervix is brought into view with the speculum. We can then only be guided by the amount of discharge, by the morbid dilatation of the internum, and by the other symptoms which I have enumerated.

"In the healthy unimpregnated uterus, as I have stated, the cavity of the uterus is only an inch or an inch and a quarter in depth, and so small as merely to contain a few drops of fluid; consequently the uterine sound once introduced has but an exceedingly limited range of motion. In internal metritis the cavity of the uterus is dilated, increased in size, and the uterine sound moves with more freedom; the presence of the sound in the uterus, and its contact with the walls of its cavity, seem also to be attended with more pain than usual. This symptom, however, cannot be much depended upon, as the introduction of the sound generally occasions pain even in the healthy uterus; not unfrequently giving rise to nausea and faintness. Indeed, the cavity of the uterus appears to be naturally extremely sensitive, whereas that of the cervix is only slightly so in most females."

"Internal metritis is nearly always accompanied by a dull, aching pain in the back or ovarian regions, similar to that experienced in inflammation of the cervix, and by deep-seated pain in the region of the uterus. The uterus is generally rather swollen, enlarged, and sensitive to the touch, the entire organ being in a congested, irritable state. Internal metritis is also often accompanied by a slight amount of febrile reaction, occurring at intervals, after exertion, instrumental interference, or at the monthly periods. The catamenia are often disordered, generally manifesting themselves more frequently and more abundantly, lasting longer, and being attended with more pain than usual. Sometimes the flow of blood is so great and so lenthened as to constitute flooding, and this is more especially observed, as might be anticipated, when the sero-sanguinolent discharge is present. With some patients, however, on the contrary, the menstrual secretion appears to be diminished; but in either case, it may be laid down as a rule, that the disease is aggravated by the appearance of menstruation. In addition to these symptoms, all the general sympathetic reactions which are observed in chronic metritis, and in chronic inflammation of the cervix, may be present. As internal metritis is generally complicated by these diseases, we may also have the peculiar symptoms which they present.

"In some rare instances, inflammation of the lining membrane of the uterine cavity is followed by ulceration. When this is the case, the cavity of the uterus becomes considerably enlarged, and large quantities of pus, blood, and mucus, collect within it, and are expelled through the os uteri. Dr Hall Davis exhibited, a short time ago, to the Pathological Society, the uterus of a woman thus affected who died under his care; there were several large ulcerations on the internal surface of the organ. There are other cases on record, but this termination of internal metritis is undoubtedly very rare. The mucous membrane of the uterus does not seem very liable to the ulcerative stage of inflammation.

From what precedes, it will be evident that although a careful digital examination, combined with the use of the uterine sound, enables us to appreciate many of the symptoms of internal metritis, yet we can only obtain all the information we require to form a diagnosis, by carefully examining with the speculum the condition of the uterine organs. The cervix should be brought completely into view, in a good light, so as to enable the medical attendant, not only to ascertain its precise condition, and that of the inferior and external portion of the cavity of the cervix, but likewise to appreciate the amount and precise nature of the discharge that issues from the os uteri."—(Pp. 59–62.)

The morbid condition of the parts in this disease is described in the following terms.

"*Pathological Anatomy.*—I have repeatedly seen the surface of the uterine cavity presenting the anatomical evidences of inflammation in patients who have died of puerperal inflammation at various periods after their confinement. The internal surface of the uterus was then red, swollen, congested, and covered with a thin coat of muco-pus; but I have only once seen a uterus presenting evidence of this form of disease in the non-puerperal state; it was in the case of internal uterine ulceration to which I have alluded. The mucous membrane presented several large inflammatory ulcerations, situated on the internal surface of the uterine walls, and quite distinct from the cavity of the cervix, which appeared free from inflammation. There was, however, considerable disease of the uterus present, besides the inflammation of its cavity. The organ was much enlarged, its walls thickened, and its cavity greatly dilated.

Dr Bennet expresses the opinion, that physicians and accoucheurs have in general entertained erroneous notions on the seat of this disease. Internal *Metritis,* he thinks, is generally confounded with acute or chronic *Metritis,* but more especially with inflammation of the lining membrane of the cervix; and the author thinks that nearly all that has of late years been written by continental writers regarding internal *Metritis,* should be referred to inflammation of the interior of the cervix. These writers, he thinks, are ignorant of the normal existence of the internal sphincter of the *cervix uteri,* as pointed out by Dr Bennet, and appear not to have formed any just view of the comparative lengths of the two cavities of the Neck and Body of the uterus. The result of this imperfect knowledge is, that in the employment of injections, which they have supposed reached the interior of the womb, and cured a morbid state supposed to be seated in that part,

they have done no more than merely applied these remedies to the *cervix* and *os uteri*, and have not in truth reached the interior of the organ.

Inflammation of the *Cervix Uteri*, or METRAUCHENIA, is the most common pathological state of this organ, according to Dr Bennet. The reason of this is, that the *cervix* contains a large amount of cellular tissue, a peculiar vascular system, making some approach to an apparatus of erectile vessels, and a mucous membrane well provided with muciparous follicles.

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The following observations contain an account of the usual modes in which the transition from health to disease is effected.

" In the married state, the cervix uteri is necessarily exposed to another fruitful cause of inflammation, even when conception does not take place. The physiological congestion and excitement which accompany intercourse may, if too frequently renewed, give rise to inflammation; and the same result may be occasioned directly by physical contusion of the organ itself. In some females the uterine system appears to be so extremely sensitive, that inflammation follows intercourse nearly immediately, even when the bounds of discretion have not been overstepped. Owing to the operation of these latter causes, many young females are attacked with inflammation and ulceration of the cervix within a few days or weeks after the date of marriage; and when such is the case, they mostly remain sterile. If they do conceive, successive abortions or miscarriages generally take place; and this will frequently be found the explanation of the repeated abortions which sometimes occur during the first years of married life, and prove so embarrassing to the practical accoucheur.

" When conception has taken place, other causes of inflammation come into action. A new life dawns on the uterus and its appendages. Instead of remaining in a quiescent condition, merely disturbed at periodical intervals by the menstrual congestion, the uterus assumes a high degree of vitality, becomes the seat of a most active nutrition, and rapidly increases in size. The hard fibro-muscular tissue of which it is formed, undergoes, apparently, a complete transformation, and assumes the decided characteristics of muscular structure; the arteries and veins, previously so small as to be demonstrated with difficulty, are developed to an enormous extent; and the entire organ becomes one of the most, instead of one of the least, vascular in the human economy. The cervix uteri participates in the change; it becomes turgid, swells, softens, and its entire structure is modified by the exaggerated organic activity which pervades the uterine system. Pregnancy may thus itself be considered a predisposing cause of inflammation of the cervix. The uterine system, however, appears to be peculiarly shielded from inflammatory action during pregnancy. Were not this the case, considering the high degree of vitality which it then presents, inflammation would necessarily be much more frequent than it actually is. A careful investigation of the morbid conditions of pregnancy has, indeed, proved to me that inflammation and ulceration of the cervix frequently exist during that state; but I believe that in these cases the inflammatory disease generally originates antecedently to conception taking place, and is merely increased and magnified by the changes which occur in the vitality of the uterus.

"Parturition is a very frequent cause of inflammation and ulceration of the cervix, as might be presumed *a priori*. Not only is parturition frequently followed by inflammation of the uterus involving the cervix, which may perpetuate itself in the latter region even when it has been subdued in the body of the organ, but it often occasions inflammation of the cervix alone, other parts of the uterine system not being simultaneously affected. This is owing to the cervix being the part of the uterus the most exposed to laceration and contusion during parturition. The cervix may be lacerated more or less extensively during the most natural labour. In a rapid confinement, a strong contraction, or a succession of strong contractions propelling the child with great force against the imperfectly dilated os, will, as I have repeatedly witnessed, thus lacerate the cervix, under circumstances otherwise the most favourable.

"The mucous membrane lining the cavity of the cervix is even more exposed to laceration and contusion than the deeper-seated structure of the organ. This mucous membrane becomes more vascular and more perfect as pregnancy advances, and as the general organic vitality of the uterus increases; its integrity being in nowise interfered with by the changes that are taking place in the uterine system.

"That such is really the case is evident, dilatation of the os uteri only commencing in primiparous women towards the end of the sixth month, and in those who have borne children not until the end of the fifth. Moreover, this dilatation of the os uteri is very slight until parturition actually commences, and is not consequently calculated to interfere with the integrity of the mucous membrane with which the cervical canal is lined. As soon, however, as the pains which precede and accompany the expulsion of the fœtus commence, the dilatation of the os uteri progresses rapidly, and in the course of a few hours is carried to such an extent as to admit of the passage of the fœtus. A necessary consequence of this rapid dilatation of a canal lined by a mucous membrane in an entire state is, that it must, in many cases, be accompanied by contusion, erosion, and laceration of the membrane. In the majority of women, no doubt, these lesions disappear promptly, cicatrization taking place with rapidity, under the influence of the retraction of the tissues of the neck, and of the reparative phlegmasia which is established, after delivery, in the cervix, as well as in the body of the uterus. But if this physiological inflammation of the uterus should prolong its duration and assume a pathological character; if remnants of the placenta or of the membranes left in the uterine cavity give rise, by their decomposition, to an irritating fœtid discharge; it is easy to understand that the lesions of the mucous membrane, instead of healing, will almost inevitably become the seat of inflammation and of subsequent ulceration.

"When inflammation and ulceration of the cervix uteri recognise this organ, it will often, but not always, be found, on inquiry, that the last abortion or labour was followed by morbid symptoms of more or less intensity, varying from severe metritis to mere uterine pains, or by a fœtid and unpleasant lochial discharge. In such cases, the inflammation and ulceration will at first exist between the lips of the os uteri or in its cavity, and if the patient is examined soon enough, the course of the ulceration may be followed as it escapes from the os, and spreads itself on the cervix. I have often met with cases of this description. In the first instance in which, a few weeks after labour, I saw a small ulceration issuing from the lips of the os uteri, I was struck with the fact, but did not attempt to explain it. The comparison which I afterwards made between cases of this description, and others examined at a later period, in which the inflammatory disease could only be traced to a natural labour, led me to perceive the clue which exists between the cause and its effect. It is, indeed, evident to me, that a considerable proportion of the cases of inflammation and ulceration of the cervix met with in practice originate in this manner."—(Pp. 68–71.)

The description of the disease follows, and is delivered at great length under the heads of Anatomical Symptoms, Local Symptoms, Functional Symptoms, and Sympathetic or Constitutional Symptoms.

The most important of these probably are that division of the Anatomical Symptoms which relates to the changes produced in the Cavity of the *Cervix*.

These are enumerated in the following manner:—

"*Changes produced by Inflammation in the Cavity of the Cervix.*—When inflammation attacks the *cavity* of the cervix, important modifications take place, both in the os uteri and in the cervical canal, —modifications which have not hitherto been described, even by continental writers. In the healthy condition, the os uteri is closed to such an extent as to be but just perceptible to the finger passing over it, and as to only admit a moderate-sized sound or bougie, which opens it in passing, as it would the urethra. The entire cervical passage, as far as the os internum, is similarly, but not uniformly contracted. When the cavity of the cervix is inflamed, it expands, on the contrary, becoming more or less open; as does also its external orifice, the os uteri, the lips of which are everted. It is difficult to account satisfactorily for the change which inflammation thus produces in the cervical cavity of the uterus. It may be owing to paralysis of the submucous muscular fibres which encircle it, induced by the inflammation of the adjacent mucous membrane; or it may be the result of inflammatory distension of the submucous cellular tissue. Whatever the explanation we adopt, the fact is certain; a more or less patent state of the os and cavity of the cervix uteri is the invariable concomitant of inflammation. This anatomical change in the state of the orifice of the uterus is invaluable in a semeiological point of view, as it can easily be recognised by the touch. Whenever the finger,—instead of passing over a scarcely perceptible orifice,—meets with a well-marked depression, into which its extremity may be inserted to a greater or less extent, we may all but conclude that inflammation, with or without ulceration, is present, and it becomes advisable to pursue the investigation farther, so as to ascertain, by ocular inspection in a satisfactory manner, the real state of the parts.

"Generally speaking, the morbid dilatation of the cervical cavity ceases before we reach the os internum, which, as I have elsewhere stated, appears in most instances to oppose a kind of barrier to the extension of inflammatory action to the uterine cavity. Should the inflammation, however, extend to the cavity of the uterus, the dilatation becomes complete throughout. It is because the distinction between the two cavities of the uterus,—that of the neck, and that of the body of the organ,—has not been made (as I stated when treating of internal metritis), that the symptoms of disease in these two regions have been completely confounded, and that many French pathologists consider internal metritis a very common form of uterine inflammation, complicating, if not originating, most cases of inflammation and ulceration of the cervix; than which nothing can be more untrue. Inflammation of the *cervical* cavity is, in reality, the common affection; whereas inflammation of the *uterine* cavity is fortunately rare."—(Pp. 74–76.)

When examined by the speculum the Mucous Membrane which lines the cavity of the *Cervix* is seen to present a dark, livid, red hue, which may be traced with the eye to a considerable depth, by depressing with the sound the lower top of the *os uteri*. This surface easily bleeds on being touched.

Though inflammation may continue for years in the *Cervix* and its cavity, without giving rise to any other anatomical changes than those which have been enumerated, this, however, is not generally the case. The mucous membrane lining these parts appears to be prone to proceed to ulceration; and ulceration is a frequent result. This generally appears first round the *os uteri*, and immediately within the *Cervix*. From this point it extends more or less, especially outwards over the cervix.

This surface may also present abrasions and excoriations, that is, destroyed parts of the mucous epidermis or epithelium. It may also present a granular appearance, a state which Dr Bennet ascribes to the circumstance of ulcerations being healed superficially. (P. 79.)

These granulations may be firm, of vivid colour, that is, we presume, bright red, scarcely bleeding on pressure; or they may be large, fungous, livid, and bleeding profusely on the slightest touch. These fungous ulcerations are generally connected with torpor of the local circulation. When they are present, the congestion of the vagina and *Cervix* is often considerable, and of a livid venous character; and the non-ulcerated surface may present dilated varicose veins, the presence of which has led French writers to give to ulcerations in which they occur, the name of Varicose Ulcerations. In pregnant females, after the first few months, ulceration of the cervix generally assumes this fungous form.

Though ulcers may be formed in any part of the *Cervix*, yet they are very frequently observed to ensue in spots occupied by mucous follicles. These bodies, from their vascular structure and condensed organization, are prone, after inflammation, to pass into ulceration. Part of their filamentous tissue swells, and causes them to project above the membrane. Their epithelial and mucous covering becomes stretched and distended. In some instances the subjacent tissue becomes dead, and the superficial covering is thrown off, disclosing then an ulcer. In other instances ulceration is the direct result of the inflammatory distension. Two or more of these may coalesce and form irregular ulcers of some size.

"In nearly all the cases in which inflammation and ulceration occupy the exterior of the cervix, they will be also found, on examination, to penetrate, more or less deeply, into its cavity. The entire cavity of the cervix, as far as the os internum, may be ulcerated. Even when the cervical canal is free from ulceration, if ulceration exists externally, it is generally inflamed to a greater or less depth. Owing to the cavity of the cervix expanding, as we have seen, when thus affected, if its lips are well separated by the bivalve speculum, and the patient is placed in a good light, the eye will often be able to follow the disease to a considerable depth; especially if one of the lips of the os uteri is at the same time depressed or elevated with the uterine sound. We must judge as to the presence of ulceration beyond the point which the eye can reach, by the nature of the secretions, and by the expansion of the cervical canal. In the cavity of the cervix it is often difficult to distinguish between inflammation and ulceration, owing to the minuteness of the granulation of the ulcerated surface."—(Pp. 80, 81.)

Another effect of inflammation affecting the *cervix uteri* is Hypertrophy. Of the origin and cause of this change Dr Bennet gives the following description.

"*Inflammatory Hypertrophy.*—Inflammatory ulceration of the cervix is generally followed, in the course of time, by important changes in the structure, size, and form of the organ. One of the first effects of the disease is, as we have seen, to produce congestion and swelling of the central structure of the uterine neck; the cervix becoming larger, but at the same time remaining soft and elastic. This state may long continue without any other change taking place. I have repeatedly found the cervix enlarged, swollen, and congested, but perfectly soft, after years of disease, especially when the disease has been limited to the cavity of the cervix or to the immediate vicinity of the os. Generally speaking, however, this is not the case. The central tissues are not only congested, but inflamed; effusion of plastic lymph takes place in their structure, and becomes more and more organized. Thus the cervix is not only enlarged, but also indurated. At first the central induration is evidently of an active inflammatory nature, as indicated by the increased heat of the organ, the vivid redness, and sometimes the pain on pressure. If the disease is not subdued, in the course of time these symptoms of inflammatory action partially subside, and the cervix becomes the seat of mere chronic hypertrophy, the inflammatory origin of which is scarcely discernible. The extent to which inflammatory hypertrophy of the cervix may be carried is perfectly surprising; the size of the uterine neck thus affected varying from that of a small walnut to that of a man's fist.

"In virgins, and in women who have had no children, the cervix seldom enlarges to any great extent. It is often indurated, although not at all increased in size, the finger detecting the induration and structural change without the eye perceiving it. When it does enlarge in virgins, the neck of the uterus seldom becomes more than two or three times the natural size, although exceptions to the rule are met with, especially with those who are advanced in life.

"In women who have borne children, on the contrary, central induration and structural hypertrophy are much more commonly met with. Owing to the greater vascularity and vitality of the uterine tissue, inflammation more readily extends to the central structure of the cervix. It is, consequently, not only more frequently followed by induration, but, when induration does occur, it is nearly always much more extensive than in virgins or even than in sterile females. It has been asserted by several French writers, that the inflammatory hypertrophy of the cervix so frequently observed in women who have had children, and who are suffering from inflammation of the cervix, is the principal cause of the ulceration which nearly invariably accompanies it; or, in other words, that the ulceration is generally a secondary affection. This assertion, however, is evidently an error. I have very often been able to follow the extension of the inflammation accompanying ulcerative disease to the deeper-seated tissues, and to watch the gradual manifestation, under its influence, of deep-seated induration. Thus I have frequently seen cases in which a slight ulceration was at first the only lesion, and in which the general induration which subsequently made its appearance gradually became more and more marked as the ulceration increased in extent. I am also continually meeting with ulceration confined to one lip, accompanied by induration and hypertrophy of that lip only. Indeed, there is generally, in recent cases, a very evident conformity between the degree of the general induration and the extent and duration of the ulceration. In the production of inflammatory induration of the cervix, there is likewise another very important circumstance to be taken into consideration, viz., the time that has occurred since an abortion or a labour. The nearer a female is to the epoch at which she has been delivered or has miscarried, when attacked with inflammation and ulceration of the cervix, the greater will be the inflammatory hypertrophy produced by the ulceration.

"The induration and hypertrophy are generally confined to the cervix; but sometimes they pass on to the body of the uterus, then, obviously, also the seat of inflammation. This is a serious complication, as it is much more difficult to restore to a healthy condition the body of the uterus when it is thus modified, than it is to overcome inflammatory hypertrophy in the cervix. Fortunately, the induration seems most frequently to limit itself to the cervix, notwithstanding the anatomical continuity of the two regions.

"Although I thus consider induration and hypertrophy of the cervix generally to be the result of the extension of superficial inflammation to the central tissues, to be the sequela, and not the cause, of ulceration, the reverse may take place. Induration and enlargement of the cervix may remain as a result of general metritis, and by the irritation which it produces give rise to inflammation and ulceration of the mucous surface. Whatever may have occasioned the general inflammatory induration, if it persist, it certainly becomes an important cause of local disease, continually reproducing the ulceration, unless means be taken to remove it as well as the more superficial disease. This it does in two ways: by keeping up a chronic state of inflammation of the organ, in which the mucous surface necessarily participates, and by the irritation which the friction of the hypertrophied and generally prolapsed cervix against the parietes of the vagina occasions.

"As the indurated cervix enlarges, the external orifice of the cervical canal, opening and expanding, assumes a transversal form; so that instead of a circular, or nearly circular orifice, we have a deep fissure, presenting well-defined lips. This is more especially the case when the induration is accompanied by extensive ulceration. These lips may or may not be equally indurated or enlarged; sometimes one is many times larger than the other. When it is the superior lip that is thus enlarged, it covers the os uteri, which the finger must search for underneath it; when it is the inferior one, the os uteri will be found above it, underneath the pubis. I have seen both the superior and inferior lip separately enlarged to such an extent as to form a kind of tumour, projecting a couple of inches beyond the less hypertrophied lip.

"The indurated cervix is not unfrequently divided into separate lobes. The presence of these lobes is an evidence of antecedent laceration of the cervix during an abortion, a difficult or instrumental labour, or even sometimes during a natural labour. The lacerated surfaces not healing, the ulceration, in the course of time, is followed by hypertrophy of the segments into which the cervix is divided. These segments sometimes assume a stony hardness, and their existence often leads to the supposition that the patient is labouring under carcinoma. I have met with several cases of this description, in which the disease had been erroneously pronounced to be cancerous by high authorities. There is, however, a means of establishing a diagnosis, which, simple as it is, has not yet been pointed out. When the lobular, knotty, irregular condition of the cervix is the result of laceration, and is simply inflammatory, the fissures which separate the lobes radiate round the cavity of the os as a centre,—which is not the case in a cancerous tumour,—each separate lobe being perfectly smooth in itself, and free from tubercles or superficial inequalities.

"Not only is this lobular form of induration erroneously considered cancerous, but even the hard inflammatory hypertrophy which I have described is still more erroneously considered to be frequently malignant by the highest and most esteemed authorities.

"*Displacements of the Cervix.*—The uterus, as I have stated, is not firmly supported by its ligaments, as is generally supposed, but merely suspended in the cavity of the pelvis, and kept *in situ* to a great extent by the natural contraction of the vagina around its lower segment, and by the pressure of the surrounding organs. Owing to this anatomical circumstance, the slightest modification in the volume and weight of the cervix gives rise to a change in its position —a fact which we have already seen exemplified in the body of the organ. Inflammatory hypertrophy of the cervix increasing considerably the specific gravity of the inferior portion of the uterus, the entire organ descends, prolapses. The cervix is thus brought much nearer to the vulva; at the same time it is very frequently directed backwards, so as to press on the posterior parietes of the vagina and on the rectum, whilst the body of the uterus may, or may not, be carried forward. This change of position, which constitutes retroversion of the neck of the uterus, is so commonly met with in married females suffering from inflammatory induration, as to constitute nearly the rule. With them it is evidently, to a great extent, the result of intercourse. In the healthy state, the cervix is soft and small, and yields to pressure; but when it is enlarged and indurated, it must necessarily offer resistance, and consequently be thrust backward, and lodged in the cavity of the sacrum. The continued recurrence of this physical cause of displacement in these cases, eventually renders the retroversion of the cervix permanent.

"Whenever there is much enlargement and induration of the cervix, unless the vagina be extremely contractile, there is always more or less prolapsus. This is more especially the case when the patient is standing; the degree to which the prolapsus may be carried depending on the amount of the hypertrophy and on the state of the vagina. If the vagina has retained its contractility—as in the virgin—it will support the uterus; but if, on the contrary, it is lax, and offers no support to the enlarged cervix,—as in women who have had many children,—it may fall as far as the orifice of the vulva, or even appear externally. This abnormal laxity of the vagina may be partly occasioned by the hypertrophy itself; the distension of the superior portion of the vagina by the enlarged cervix diminishing its tonicity; and the uterus then falling, as it were, into a non-contractile pouch. When it thus lies low in the vagina, it gives rise to a very irksome sensation of weight, dragging, and bearing down, which may be felt, not only in the pelvic region, but in the abdomen, the patient often feeling, especially when erect, as if a foreign body were about to escape from her. These sensations are occasioned partly by the weight of the uterus bearing anomalously on the floor of the pelvic cavity, and partly by the traction which the enlarged and prolapsed womb exercises on its ligaments, and on the organs with which it is connected. When sitting or lying, the bearing-down sensation is less marked; but if the enlargement of the cervix is considerable, there may be another sensation experienced, that of a tumour, pressed up, when sitting, by the resistance of the seat.

"The hypertrophied cervix is sometimes directed anteriorly, or anteverted; it then lies behind the pubis, more or less high, according to the extent of the anteversion. When this is the case, it is always owing to some enlargement of the body of the uterus, which causes the uterus to fall back into the cavity of the sacrum, and thus throws up the cervix.

"The hypertrophied cervix occasionally lies diagonally in the pelvic cavity, to the left or to the right; so that the finger passed into the pelvis per vaginam in a straight line towards the sacrum, misses it entirely, leaving it on one side. When the cervix is directed to the left, as is usually the case, I scarcely consider the displacement morbid. In many non-pregnant females, as I have already stated, the uterus naturally lies diagonically from right to left, and in the cases

> in question this position is merely exaggerated and rendered more apparent by the hypertrophy."—(Pp. 84–89.)

As inflammatory ulceration of the *cervix uteri* is not by itself in all instances a fatal disease, opportunities of ascertaining after death the changes produced by it during life are not frequent, and take place chiefly when patients under it, die in consequence of some accidental disease. In such circumstances, Dr Bennet has found only those anatomical modifications which ocular inspection of the parts during life would lead the observer to anticipate. Where ulceration is present, the mucous membrane is either slightly corroded or entirely destroyed. In the latter case, the fibrous structure of the subjacent parts becomes distinctly visible, being dissected, as it were, by the process of ulceration. The ulcerated surface itself is not excavated, but on a level or nearly so, with the surrounding tissues; the margin being perfectly smooth and regular, and presenting no jagged, hardened indentations. The *cervix* itself when chronically enlarged, presents all the characteristics of cellular hypertrophy, its tissue being more dense and more resisting than in the normal state.

The next Chapter, the Sixth, may be regarded as presenting the enumeration and development of those pathological principles which peculiarly distinguish the treatise of Dr Bennet. Hitherto inflammation and ulceration of the CERVIX UTERI has been viewed as a substantive affection. Here it is considered in its connection with LEUCORRHŒA, DYSMENORRHŒA, AMENORRHŒA, Irregular Menstruation, Partial Prolapsus, and similar displacements of the Uterus. The affection is accordingly considered more particularly as it takes place in the virgin female.

According to Dr Bennet, the existence of inflammatory ulceration of the Uterine Neck is a disease of not unfrequent occurrence, was totally unsuspected by all who had written on uterine diseases previous to the time, 1845, at which Dr Bennet published the first edition of his work; and even the author himself spoke at that time of the lesion in terms of doubt and hesitation. He adduces at the same time, from that edition, a passage with the view of acknowledging that the passage contains an error which he now desires to rectify.

> "The experience of the last few years has shewn me that the above extract contains an error which I have now to correct. Not only *may* inflammation and ulceration of the uterine neck exist in the virgin female, but it *does* exist, and not *very* unfrequently, if I

may judge by the results which consultation and dispensary practice have afforded me.

"When I wrote, finding nothing on inflammation and ulceration of the neck of the uterus in the virgin, in any of the authors who preceded me; never having heard a remark on the subject escape from the eminent Parisian pathologists whose pupil and assistant I was for many years, and not having met with this form of the disease myself in hospital or private practice—or, at least, not having recognised it,—I concluded that when ulceration did exist, it healed spontaneously,—as is often the case in the mouth,—owing to the patient not being exposed to the causes of irritation which obtain in the married condition. Reason told me that the cervix uteri must occasionally become inflamed and ulcerated; but for want of the experience which I have since acquired, I was obliged to surmise that the cure was always, or nearly always, spontaneous. It will be seen, however, by the very guarded manner in which I wrote, how unwillingly I came to this conclusion, and that I foresaw, as it were, the results which subsequent research has developed.

"For the last few years I have very carefully analysed the state of all the young unmarried females presenting uterine symptoms for whom I have been consulted, with a view to elucidate this very important question, and have thus ascertained, in the most positive manner, that inflammation and ulceration of the cervix uteri in the virgin are not an uncommon disease, and that to it may be referred most of the severe forms of dysmenorrhœa which resist the ordinary modes of treatment, and most of the cases of inveterate leucorrhœa in the virgin, which are connected with great general debility and prostration.

"Not only have I frequently met with inflammatory ulceration of the cervix in virgin females above twenty, who have menstruated for some years, but I have, in several instances, discovered the disease existing in a most decided form in young females only sixteen and seventeen years of age, in whom menstruation was not even yet fully established. I have now two cases of this description under my care, which I shall give at the end of this Chapter. They shew, most satisfactorily, that the congestion which precedes and accompanies the establishment of the function of menstruation in the female economy may become morbid, and be followed by the development of ulcerative inflammation. As yet I have had no reason to suppose that the neck of the uterus is ever ulcerated previous to the age at which menstruation appears. Considering the dormant condition of the uterus when it has not yet been roused into functional activity, I should think it is scarcely likely then to take on severe inflammatory action.

"This discovery cannot but be considered of extreme importance, inasmuch as it brings at once within the scope of successful treatment

> a class of most distressing and intractable cases. At the same time it must also be admitted that it very much increases the delicacy and difficulty of their management. The manual and instrumental examinations imperatively necessitated by the presence of extensive physical lesions in the deep-seated uterine organs, are at all times repugnant to female delicacy, and their proposal, under any circumstances, can only be warranted by the serious nature of the case; but the scruples of the medical practitioner must be increased tenfold when the sufferer is a virgin female. If, however, he is satisfied that his patient is labouring under a disease which is destroying the very sources of health, and the disastrous effects of which can only be arrested by physical examination, it would be a dereliction of duty, as well as a false and culpable delicacy, not, if possible, to overcome all obstacles, whatever may be their nature."—(Pp. 130–132.)

It cannot, we believe, now be denied, that Inflammation and Ulceration of the *cervix uteri* often exist in young females in connection with LEUCORRHŒA, AMENORRHŒA, DYSMENORRHŒA, and probably other affections of the uterus. These lesions, however, may either coexist with, without being the cause of, the deranged menstrual function, and the cause of the malposition of the uterus; or both these lesions and the derangements in the menstrual function may proceed from the same general causes. We presume it can scarcely be argued with any show of justice, that *Leucorrhœa, Amenorrhœa, Dysmenorrhœa, Menorrhagia,* and similar disorders are causes of inflammation and ulceration of the *cervix uteri.* At the same time it appears, that a reasonable amount of evidence has now been adduced to shew, that inflammation, and occasionally ulceration of the cervix uteri, are either antecedents or accompaniments of the conditions of disordered menstruation now mentioned. Dr Bennet speaks with great confidence as to these lesions being causes, if not invariably, at least so frequently, of disorders in menstruation, that the medical practitioner who is consulted for the relief of such disorders, would neglect his duty, and of course injure his character, if he did not in every given case take means to ascertain the fact of relationship or not.

> "In many of the instances which I have seen of ulceration in the virgin female, the most prominent symptom has been dysmenorrhœa in a very severe form. Indeed, as I have stated above, I am convinced that most of the cases of extreme and obstinate dysmenorrhœa and disordered menstruation, which are at last considered hopeless, and are merely palliated by narcotics, will be found, on careful scrutiny, to be cases of ulcerative inflammation of the uterine neck.
>
> "When the cervix is inflamed and ulcerated, the menses, whether they have previously been easy or difficult, generally become pain-

ful, sometimes agonizingly so, all the local pains being much exaggerated. It is not, however, the existence of pain during menstruation, as we have seen elsewhere, that indicates the presence of ulcerative disease, some women always suffering pain, even in the absence of uterine inflammation, but the presence of pain when it did not previously exist, and its increase when it did. Amenorrhœa or menorrhagia are also frequently observed. The breasts are often sympathetically affected; they become large, swollen, tender, and painful, and the areola is developed as in early pregnancy."—(Pp. 134, 135.)

The only mode of obtaining positive information is sufficiently manifest.

"*Physical Examination.*—A satisfactory digital examination of the uterus may be nearly always made in a virgin, without injury to the hymen, especially when the vagina and external genital organs have been relaxed by long-continued congestion and inflammation. The hymen is nearly always sufficiently dilatable to admit the index, introduced slowly and with proper care. Generally speaking, the os and cervix are reached with ease, the inflamed cervix not being retroverted, as it is in most married females; and when once the finger has reached the os, in most cases, all doubts are solved. If the cervix is free from disease, it is soft, and the os is closed; if inflamed and ulcerated, it is enlarged and swollen, and the os more or less open and velvety. This open, soft state of the os and cervix may also exist from mere inflammation of the cavity of the uterine neck. If the uterus, however, lies diagonally, from right to left, there may be some difficulty in reaching the cervix.

"When the existence of ulcerative disease of the uterine neck has been thus recognised in a virgin, what course must we follow? As it may react so disastrously on the female economy as absolutely to endanger, indirectly, the life of the patient, not to speak of its making her a burden to herself and to all around her; as, likewise, when the disease is severe and confirmed, all non-instrumental means of treatment are totally inefficacious, there can be no room for hesitation. The speculum must be used, if possible without dividing the hymen; but if its introduction is otherwise impossible, the hymen must be carefully divided.

"In many cases, as I have before stated, the hymen is naturally very lax, or has been relaxed by disease; I have therefore had a very narrow, small, bivalve speculum made, with which I am generally able, by degrees, and with a little time and patience, to dilate it, and thus to examine the patient, without any preliminary division."—(Pp. 135, 136.)

Dr Bennet speaks with considerable confidence of the number of cases in which ulcerative inflammation had existed in young females, causing great suffering and making most hurtful inroads upon the general health, yet without being suspected, and of course treated by improper or useless remedies, until, as we are given to understand, he had discovered the true cause of their complaints and adopted the remedies accordingly. Again he seems desirous not to overstate the case; and accordingly the following cautionary and limiting admissions are made.

> "Notwithstanding all that I have said above, I must not be considered to assert that this disease is a very common one in the virgin female. On the contrary, I believe it to be exceptional; but I also believe that all practitioners engaged in the consultation-practice of uterine disease will recognise it frequently, as I do, if they bear in mind the facts which I have pointed out. I may say, without exaggeration, that since I called the attention of the profession to the existence of the disease in virgins, not a month has passed without my being consulted in cases of severe inflammatory disease of the cervix in virgins, having resisted years of general treatment; and that I have thus been instrumental in restoring to *perfect health* many young females who, when I first saw them, were mere wrecks, and had lost all hope of recovery. When such a disease is once known to exist it would be an opprobrium to medical science to allow it to remain unchecked from motives of false delicacy."—(P. 139.)

The Chapter is concluded with the details of four cases of different forms of ulceration of the *Cervix Uteri*, in which that disease had induced *Amenorrhœa, Dysmenorrhœa, Leucorrhœa.*

Dr Bennet, it must farther be mentioned, states, that he is in a position to prove, by statistical data, the fact that inflammation of the lower portion of the uterus is truly as frequent an occurrence, and performs as important a part in uterine pathology as he had represented that it did in 1845. During the last few years he has kept a careful register of all the instances of uterine disease which he treated at the Western General Dispensary, to which institution he is physician-accoucheur. The Western Dispensary is one of the largest institutions of the kind in London, nearly ten thousand patients being annually treated by its medical officers. The patients seen by Dr Bennet consist of those who present uterine symptoms, and are either addressed to him by colleagues or by the house-surgeon, on registration. He finds on analyzing his cases, three hundred in number, whom he had attended professionally between the 1st of July 1844 and December 1848, that two hundred and forty-three were suffering under decided inflammatory disease of the uterine cervix or its cavity; and that in two hundred and twenty-two ulceration was present.

At the end of the volume, a tabular view of these three hundred cases is given, shewing all their details and individual characteristics. This table therefore furnishes good evidence of the correctness of some of the doctrines of Dr Bennet.

It would be wrong, perhaps presumptuous, to whisper any doubts about the reality of the connection for which Dr Bennet argues, certainly with considerable ingenuity. But we must say, that at times we have doubts whether the lesions now specified, and the disorders in menstruation and in the function of the uterus actually stand to each other in all instances, in the relation of cause and effect. One observation upon this point we have to offer. We have had occasion to see and to treat not a few instances of females labouring under either AMENORRHŒA or DYSMENORRHŒA, with more or less LEUCORRHŒA; and occasionally married females either sterile or liable to abortion. In some instances the menstrual disorder disappeared under the use of remedies, and the patients recovered health. In other instances the menstrual function was not re-established, or in any way amended. In all such instances, however, the patients were eventually attacked by other disorders; in some instances by phthisis, in some by dropsy, in some by disease of the heart, in one instance by mental derangement, and death followed exactly as it does in similar cases. It seems scarcely reasonable to say, that the ulceration of the *Cervix Uteri*, if it existed in such cases, could have induced any one of the distempers now mentioned. But we think it is a conclusion not unreasonable, nor at variance with facts, to say, that both depended upon the same general cause; some undefined and undescribable weakness of the system, the same probably which causes consumption, scrofula, and similar distempers.

The weak point in the doctrine of Dr Bennet, if we may be allowed to offer any opinion, appears to consist in this, that he makes a local disorder to be the essential and uniform cause of all the functional derangements, and several of those that are organic, to which the uterus is liable. It is not impossible that this view may be right. But if it be, it is at variance with those doctrines which have long seemed to rest on the best foundation, and which are daily becoming more strongly confirmed, namely, that local disorders and diseases affecting the functions and structure of certain organs, owe a large proportion of their origin, if not the whole, to some taint or distemper in the system at large, some morbid element which probably resides in the blood, and is either coeval with the beginning existence of the individual, or the germs of which are coeval with that period, and which are merely called into an active state as life becomes more developed and then manifest their action upon one organ or several organs, according to the circumstances in which individuals may happen to be placed.

We have seen what is the doctrine of Dr Bennet regarding the origin of disorders in the Uterine and Menstrual Functions. The main cause of all these is some form or degree of inflammatory congestion and ulceration of the *Cervix Uteri.*

Somewhat different from this is the doctrine taught by Dr Tilt. According to this writer, as our readers are already aware, AMENORRHŒA, DYSMENORRHŒA, LEUCORRHŒA, MENORRHAGIA, and even HYSTERIA, all owe their existence to some form or degree of inflammation in the ovary or ovaries, by which the influence, which, in a state of health, these organs exercise over the circulation of the uterus, is suspended, perverted, and destroyed. As upon a former occasion, when noticing the doctrines proposed by Dr Tilt, they were then stated at sufficient length,[1] it is not necessary at present to do more than merely mention them, and refer to the volume in which they are stated, while it is not easy to overlook the opposition in which they stand to those of Dr Bennet. If the doctrines proposed by Dr Tilt be well-founded, it follows, almost as a necessary consequence, that the inflamed and ulcerated states of the *cervix uteri* cannot be necessarily connected with the symptomatic conditions of *Amenorrhœa, Dysmenorrhœa, Leucorrhœa, Menorrhagia,* and that the connection, if it exist at all, can be only accidental and coincident. If, on the other hand, it be the fact, that these disorders in the Menstrual Function are connected with inflammation and ulceration of the *Cervix uteri,* it must still remain a question, to what extent the condition of the Ovaries, inferred by Dr Tilt, to cause these menstrual disorders and impediments, may or may not be connected with the inflamed and ulcerated state of the uterine neck likewise.

Dr Bennet proceeds, in the Seventh Chapter of his work, to direct the attention of his readers to another of his peculiar and leading doctrines; namely, the Influence exerted by Inflammation and Ulceration of the Neck of the Uterus during Pregnancy, in causing laborious, that is, painful and uneasy Pregnancy, Hæmorrhage, Obstinate Sickness, Death of the Fœtus, Moles, Abortion, and all those evils which attend these accidents.

Dr Bennet enters into a detailed history of the steps by which he was led, amidst the facts furnished by the experience of an Hospital-Prison in Paris, gradually but confidently to form the opinion of the great influence which these morbid states of the Uterus exert in causing the effects specified. We believe that it requires little reasoning, nevertheless, to satisfy any one trained to a reasonable amount of physiological knowledge, that ulcers on the neck of the uterus, or even inflammation without ulcers, are adequate to produce abortion. If any part of the organ is so diseased, this must seriously interfere with the healthy and normal circulation of the rest; and if the uterine circulation be disordered or disturbed, even in a small degree, from its healthy state, it surely requires little reflection and reasoning to shew, that this cannot fail to affect the connecting vessels between the Womb and Placenta, and in this manner to affect the life of the Ovum. This, therefore, we conclude, must be admitted as almost indisputable.

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[1] Edinburgh Medical and Surgical Journal, vol. lxxvi., October 1851, p. 470.

This, however, touches not the whole merits of the question; which is, are there no instances of laborious Pregnancy, Hæmorrhage, death of the Fœtus, Abortion, Moles, and similar evils, in which the uterine neck is in a healthy state. In every instance of death of the Fœtus and abortion, is it proved, that inflammation and ulceration of the uterine neck are adjuncts?

It is well known that Mr Whitehead of Manchester adduced in his work, published in 1847, decided evidence to shew, that ulcers in the neck of the uterus are very frequent occurrences among the females in the city of Manchester, and that abortion and sterility are very frequent consequences of ulceration of the uterine neck. But Mr Whitehead at the same time shewed, that besides inflammation and ulceration of the uterine neck, a considerable number of morbid states in the uterus, and of the uterus and its appendages, tended to produce these unfavourable results. It is unnecessary to return to the instructive volume of Mr Whitehead on this occasion, and to repeat what is there said. But we subjoin a few of the remarks of Dr Bennet on this subject, merely to give our readers just notions of the opinions entertained and the conclusions formed by him upon this subject.

> "When inflammatory ulceration of the cervix exists during pregnancy, a minute inquiry into the previous uterine history of the patient will generally prove that it existed *previous* to the pregnancy. I formerly believed that the disease mostly originated subsequently to conception. This opinion, however, subsequent experience on a wider field has shewn me to be erroneous. Although it sometimes thus originates, in the great majority of cases it is evident that the cervix is diseased previous to conception. The recognition of this fact has

necessarily led me to modify my opinion with reference to the influence of inflammatory ulceration of the uterine neck as a barrier to conception. In most instances it has this effect, rendering women sterile who have never conceived, and arresting conception in those who have. This rule suffers, however, many exceptions, especially with the latter class of females. The disease generally produces sterility when it attacks young married females at the onset of their married life, but does not so generally arrest conception when they have already conceived, and have previously had children."—(P. 158.)

The state of the Uterine *Cervix* and *Os* when so diseased, during pregnancy presents peculiar appearances. When the Cervix is brought fully into view, it is found swelled, congested, livid in colour, voluminous, soft, or only partially indurate; and on one or both lips, generally penetrating within the cavity of the *Os uteri*, is seen a more or less extensive ulceration, sometimes covered with large fungous granulations. This growth of granulations, this luxuriant fungous production from the ulcerated surface, is so marked in some cases, and so seldom observed in the non-pregnant state, that when it is found, it may be said alone to constitute a symptom of the presence of pregnancy. The ulcerated surface is, in general, covered with muco-purulent matter, and bleeds readily, owing to the luxuriance and vascularity of the granulations. Its fungating character is, in some instances, so strong that it might give rise in the minds of those not accustomed to such appearances, to the idea, that the patient is affected with malignant ulceration of the Uterus. This fungous character, Dr Bennet has generally observed, to begin to make its appearance in the *Cervix Uteri* of pregnant females, about the end of the third or fourth month of pregnancy. The vagina often presents distinct hypertrophy of the mucous follicles.

Of this state and the concomitant one of hypertrophy, hæmorrhage is a frequent result. It is not the same, however, as that which precedes and causes abortion. It proceeds from a different region of the generative organs; and though the ulceration may cause abortion, hæmorrhage may often take place in this manner, and not be followed by abortion.

The symptomatic effects of this state, as of most others, involving the uterine neck, are great debility, racking pain in the back and loins, squeamishness, and vomiting, and other symptoms of gastric disorder.

The existence of ulceration in this part is always an unfavourable circumstance, as it renders the patient greatly more liable to metro-peritonitis at and after parturition.

Four examples of the disease are given, shewing its effects in causing abortion,—in two of which abortion was prevented by the employment of suitable treatment.

Dr Bennet next considers, in Chapter Eighth, the influence of inflammation, ulceration, and induration of the neck of the Uterus during and after Parturition, in connection with rigidity of the *Os uteri* during labour; with Flooding; and with the morbid symptoms that follow natural and difficult labour.

In Chapter Ninth he treats of Inflammation and Ulceration of the neck of the Uterus in advanced life after the cessation of Menstruation.

The Tenth Chapter is devoted to the subject of inflammation of the Vulva, of the Vulvo-Vaginal Gland, and of the Vagina.

The most effectual mode in which we can convey to our readers an idea of the manner in which the author considers this part of the subject, is by placing before them the following passage.

"When the inflammation occupies the muco-cutaneous surfaces, we find the vulva red, hot, congested, swollen, tender to the touch, and bathed with mucus at first, and subsequently with muco-pus. If it has extended to the cellulo-erectile tissue underneath, the labia and nymphæ often become enormously swollen, so as to present the form of large masses on each side of the vulvar orifice, which appears greatly enlarged. When this is the case, purulent collections may form either in the free cellular tissue or in the larger mucous follicles, and especially in the vulvo-vaginal gland.

"The numerous mucous follicles of the vulva are sometimes inflamed separately, or at least their inflammation becomes the prominent feature, that of the tissues in which they are embedded being secondary. At first they present the appearance of numerous small hard specks of coarse sand disseminated over the muco-cutaneous surface. As inflammation progresses they often ulcerate, and then the parts are dotted over with small aphthous-looking sores, secreting muco-pus in abundance. This state of things is generally attended with considerable inflammation and swelling of the surrounding parts, which are matted together by the purulent secretions, and present a very repulsive appearance.

"Dr Oldham has well described a chronic form of follicular inflammation occasionally met with, in which the inflammation principally attacks the mucous follicles of the nymphæ and of the vaginal orifice extending from the meatus to the lower commissure of the nymphæ, and seldom involving, to any extent, the external labia. The small aphthous ulcerations which they form, at first sight rather resemble venereal sores. On a closer inspection, however, their purely inflammatory nature becomes evident. The presence of this chronic follicular inflammation is often attended with spasm of the constrictor vaginæ, and consequent occlusion of the vaginal orifice. Thence extreme pain on any attempt at congress. This form of the disease is generally most intractable to treatment. It may exist independently of any vaginal or uterine inflammation, but has proved in my practice mostly connected with such disease. Owing to the spasmodically constricted state of the vaginal orifice, it is very difficult satisfactorily to examine the vagina and neck of the uterus, either digitally or instrumentally.

" Vulvar inflammation especially in the chronic form, is frequently accompanied by intense irritation and itching. It may be general in the vulvar region, involving or not the clitoris, or it may be confined to the clitoric region alone. This symptom is a most distressing one, often destroying entirely the rest of the patient, and when carried to an extreme degree rendering her nearly frantic. She is induced, in spite of the strongest determination to the contrary, to rub the part affected, in order to allay the itching, and thus the inflammation is increased, while the local irritation is but temporarily relieved.

" On a careful examination, these chronic forms of vulvar inflammation will generally be found connected with extensive disease of the cervix uteri, and this partly accounts for their extreme intractability to treatment, especially when this is directed to the vulvar element of the disease only, as is usually the case, the disease of the uterine neck, which keeps up the external inflammation, being unrecognised and unchecked."—(Pp. 207, 208.)

On the subject of *Vaginitis*, and a cognate question, upon which we had occasion to offer some remarks, when examining the treatises of Mr Acton, Mr Skey, Mr Langston Parker, and Mr Milton, the following observations are made.

" It is a singular pathological fact that, although the existence of a specific and contagious form of vaginitis is generally admitted; yet that it is difficult, if not impossible, as we have seen, to point out any decided characteristic by which it may be distinguished from simple vaginitis. Like those who have preceded me, I am unable to indicate satisfactorily any absolute means of distinguishing between simple inflammation of the vagina and blenorrhagic inflammation, although I believe that the difference does exist. This seems proved to me by the fact that simple inflammation of the vulva and vagina, the form which is so constantly found coexisting with disease of the neck of the uterus, and the origin of which is evidently inflammatory, does not appear, as a rule, to communicate gonorrhœa to the male. I not unfrequently hear of the husbands of my patients suffering from slight irritation, but seldom of their having positive urethral inflammation with purulent discharge. The instances of the kind which I meet with—instances in which, although the wife's disease appears to be of a purely inflammatory nature, yet the husband is obliged to live separate, under penalty of being himself attacked by urethritis—are, indeed, so rare, that I can only look upon them as exceptional. For many years my opportunities of observing uterine disease have been principally confined to the moral classes of society; for even when physician to a dispensary, my patients were mostly the wives of respectable artisans; and I have been greatly struck to find, as a rule, the husbands of my uterine patients living with their wives in apparent immunity, although nearly all of them are, and have been, suffering for months or years, when I first see them, from vaginitis of a more or less severe character. It may be that they become acclimatized, as it were, to their wives' local state of health, or it may be that I do not hear what takes place; but it is even more probable that the immunity is real, and the result of the non-contagion of purely inflammatory vaginal discharge under ordinary circumstances. I say under ordinary circumstances; because, even admitting that such is the case, we can easily understand that a morbid secretion, innocuous when brought into contact with a healthy frame, may, on the other hand, produce violent inflammation, if the economy is below par, or if any urethral irritation, caused by lithatic urine or any other cause, previously exist; or if the patient is debilited by excesses of any kind.

"To appreciate all the bearings of the question, the above facts must be compared with the results furnished by the medical history of the non-moral part of the population. Do we not find that the young and unmarried who associate casually with women of loose character, with women who do not offer moral guarantees, are continually attacked with gonorrheal inflammation; a fact which tends to prove that they are often exposed to a contagious element in the one case that does not obtain in the other, although the physical evidences of inflammation are identically the same to the eye in both instances?

"Although, thus believing in the existence of a contagious and specific form of vaginitis, I am bound to confess that the only difference that I can see between the two is, that vaginitis contracted by contagion, or blennorrhagia, appears to me to be more acute than ordinary vaginitis, that there is a greater quantity of pus secreted, greater redness, congestion, and swelling of the mucous membrane, and that the inflammatory action has a greater tendency to spread to the urethra, and is very much more intractable to treatment. These conditions, merely implying degrees of inflammatory violence, do not evidently constitute a distinction as to morbific characteristics."—(Pp. 218, 219.)

These observations merely contain an acknowledgment of the extreme difficulty of distinguishing between instances of simple vaginal inflammation and discharge, and that which is the result of a supposed poisonous principle or blennorrhagia.

Our limits, however, permit us not at present to dwell longer upon the contents of the present volume. Several of the most important of the propositions advanced by the author are yet to be considered, more especially his account of *Oaritis* Acute and Chronic, his directions for treatment, his description of *Dysmenorrhœa, Amenorrhœa, Menorrhagia,* and *Leucorrhœa;* his history of the Causes of Sterility; his account of the Displacements to which the Uterus is liable; Tumours of the Uterus, and Cancer of the Uterus. The examination of these subjects must be deferred to a subsequent occasion.

Meanwhile it is due to Dr Bennet to say, that he has in the present volume given a very complete view of the questions relating to the propositions which he advocates; and the evidence which he adduces will certainly be most serviceable in enabling practitioners to judge for themselves, to what extent these propositions are supported by facts. It is quite unnecessary at present to express any opinion on the validity of that evidence. The great point is to direct to it the attention of physicians and surgeons. The question is not one which can be decided either by the number of the opinions given, or by the authority of those by whom the opinions are given. It is only by subjecting them to that discussion which successive years furnish, and the knowledge produced by successive years, that these questions can be determined. To this tribunal therefore we leave them.

The work of Dr Tilt is a greatly enlarged form of his first performance, and contains much additional information, and much corroborative evidence of the justice of his doctrines. A considerable amount of new matter upon the Natural History of the Menstrual Function, upon the influence exerted by the Ovaries, and upon that exerted by the Ganglionic System, is introduced; so that the Treatise exhibits a connected view of the entire subject. As such it may be recommended to the attentive consideration of all orders of the profession.

ART. II.—1. *Practical Remarks on Some Exhausting Diseases.* By Sir JAMES EYRE, M.D., Licentiate of the Royal College of Physicians; Senior Physician to the St George and St James's Dispensary. Second Edition. London, 1851. 12mo, pp. 110.

2. *The Stomach and its Difficulties.* By Sir JAMES EYRE, M.D., Edinburgh. Licentiate of the Royal College of Physicians of London, &c. London, 1852. 12mo, pp. 152.

The first of these volumes appeared originally in 1845; and soon after, an account of the work, and the effects of the remedy which the author recommended, was given in the sixty-fourth volume of this Journal (July 1845).

Since that period the author has been occupied in collecting further evidence on the therapeutic merits of the oxide of silver in the treatment of Hæmoptysis, Menorrhagia, Hæmaturia, Epilepsy, and some other diseases accompanied with much debility; and all the facts concur in shewing that oxide of silver, properly and judiciously administered, is a serviceable remedy.

In the treatment of Epilepsy it appears to exert more beneficial effects, and more decidedly, than the nitrate of silver; and the evidence adduced by Sir James Eyre is certainly entitled to attention.

The second performance,—on the Stomach and its Difficulties,—is new. Although the subject of stomach diseases and complaints is one which gives rise to a very great number of writings, it will always command a certain amount of interest with different classes of society. The present work by Sir James Eyre is not lengthened; and this is, upon such a subject, a great recommendation. The author communicates in an agreeable manner a large amount of useful observations upon the causes of indigestion, and the most effectual mode of curing the disorder. He strongly enjoins moderation and temperance in the use of articles of food and drink; and shews that he is well aware that, without observing the rules enjoined by these authorities, medicine is of very little use.

Sir James Eyre further adduces in this volume good evidence in favour of the use of the oxide of silver, given in half-grain or one-fourth-grain doses, in certain forms of Dyspepsia, probably nervous; in Gastrodynia and Pyrosis; in Epilepsy, and in Chronic Diarrhœa. One of the correspondents of the author has given in Epilepsy doses of one grain, and two grains of the oxide; but the usual doses are one-fourth of a grain, one-third of a grain, half a grain, and three-fourths of a grain.

From a statement made in page 113 of this second publication, it appears that the use of oxide of silver is free from any effect in blackening the skin. If this statement be confirmed by subsequent observation, it will form a strong recommendation for the preference of this metallic oxide over the nitrate of silver, in attempting the cure of epilepsy, chorea, and similar disorders, in which the use of the remedy requires to be continued for a long time.

We observe that the use of Oxide of Silver in the Treatment of Epilepsy, is strongly recommended by Mr Solly in his work on the Brain.

The work forms altogether a valuable contribution to the therapeutics of Gastric Disorders.

ART. III.—*Some Account of the last Yellow Fever Epidemic of British Guiana.* By DANIEL BLAIR, M.D., Surgeon-General of British Guiana. Edited by JOHN DAVY, M.D. F.R.S. Lond. & Edin., Inspector-General of Army Hospitals, &c. London, 1850. 8vo, pp. 161.

Guiana is a sea-coast territory in the northern part of South America, lying on the north of the equator, between the first degree and the tenth degree of north latitude. Originally the river Orinoco was understood to be its northern boundary, and the mouths of this river join the Atlantic under the ninth and tenth degrees of north latitude. The name of Guiana, however, is now confined to the district situated between the Essequibo River in eight degrees north latitude on the north-west, and which separates Guiana from Colombia,—and the River Carapuri, which divides it from Brazil on the south-east, or, according to some, by the Amazon. The line of its coast runs north-west and south-east about the length of six hundred miles, and the average breadth from this coast line into the interior is about two hundred and fifty miles. Of this tract the south-eastern portion belongs to the French; the middle portion to the Dutch; and the north-western to the British. The latter division, or British Guiana, comprehends three subdivisions, Berbice, Demarara, and Essequibo.

The entire tract is exceedingly flat and alluvial, intersected by rivers, the Essequibo, Demarara, and Savanna, and requires dikes and embankments to prevent the encroachments of the rivers and the sea, and canals, in order to carry away the superfluous water.

British Guiana, in its colonized portion, is chiefly distinguished by its flatness and lowness of level, which is below that of the sea at high water during spring tides, and by the clayey nature of its soil, which is nearly destitute of lime, and is more or less charged with oxide of iron and sea salt. The entire surface is remarkable for the never-failing luxuriance of its vegetable products. The inhabitants reside principally along the skirts of the seashore and at the mouths of the large rivers, Demarara and Essequibo. As the level is below that of the sea, the cultivated lands require embankments on all sides. Sluices in these embankments, at suitable times, give issue to the surface-drainage of the land. The ordinary rise and fall of spring tides is nine feet. The district is intersected by numerous drainage and navigable canals. Within the city of Georgetown in 1837, besides the private drains, there belonged to the public thirty-one miles of open trenches, varying in width from two to ten feet, communicating by sixty-two tunnels, and these required to discharge the surface water alone.

Originally settled by the Dutch in 1706 on the Essequibo, and in 1746 on the Demarara, the coast and soil were treated very much after the manner of Holland. The sea-coast districts were, by the erection of dikes and the formation of canals, converted into Polders, while the creeks and low savannahs give it the appearance of immense meadows of land, intersected by canals and pools of water. Placed under the equator, its temperature is uniformly high, and the atmosphere as well as the soil is humid. The coast line, which extends nearly east and west, is cooled and dried during the greater part of the year by the trade-winds.

About the centre of the sea-coast of British Guiana, near the mouth of the Demarara River, stands Georgetown, the ancient Stabroeck of the Dutch settlers, the capital of the colony, having Essequibo on the extreme west or leeward of the coast; and Berbice, with its chief town and seaport, New Amsterdam, on the extreme east or windward part of the coast.

Georgetown is laid out in rectangular streets, generally of great width, and is composed chiefly of wooden houses, which are raised on pillars several feet above the surface of the ground, and which are separated from each other by gardens, containing shrubs, and fruit and ornamental trees. The houses with open windows, jalousies or blinds, and galleries, may be considered as performing the duty of large umbrellas to protect the inhabitants against the heat of the sun, and the wetting of the tropical rains. In 1837, when Georgetown contained less than 20,000 inhabitants, it covered an area of two miles. This spaciousness of the city is supposed to be rendered necessary in order to prevent fires, and to promote ventilation.

Georgetown, however, possesses two divisions devoted to commercial pursuits, namely, Water Street and Robb's Town which are exceptions to the description now given. There, the houses are nearly in contact, and instead of the construction upon pillars with open intermediate spaces, or uninhabited cellars, the ground floors are used and occupied during the day as shops or stores. In this portion of the city the yellow fever epidemic of 1837 was most prevalent, as it was in America Street in 1819, then the mercantile part of the town, that the epidemic of that period chiefly prevailed. Robb's Town has no peculiarity, except that the houses are closely compacted together. Water Street has peculiarities entitled to notice. Its highway is formed by the embankment that prevents the overflow of the River-Tide; and one side of the street, comprehending stores, dwellings, kitchens, stables, &c., is built on the bed of the river itself, being supported above the level of the water by piles and platforms. This side of the street is designated by the name of the Mud Lots of Water Street. To almost every one of these Mud Lots is attached a landing wharf, called after the names of the Dutch settlers a Stelling or Placing,[1] composed of piles with a platform, from four to six feet broad, and extending beyond the buildings into the shelving clay-bed of the river. Similar Stellings are still seen on the banks of the rivers of Rotterdam, Amsterdam, and other trading towns of Old Holland. Seven public Stellings, at several intervals, maintain a free communication between the city and the shipping. Across Water Street six sluices discharge the contents of the drains and sewers past the Stellings into the river.

At the angle formed by the river and ocean embankments, and to the north of the city, are situated the Military Grounds. To leeward of the City, and out in the River Stream at prescribed lines of distance from the shore, lie the mercantile shipping.

By the system of Stellings or Wharf-Places over the Mud Lots of Water Street, the current of the river, for about two miles in length, is subjected to a kind of coarse filtering, in which bulky materials of any kind, floating among the piles, are liable to be entangled and detained below the Stellings. A short time previous to 1837, a custom was commenced with the public Stellings, and extended to those of private individuals, of filling a portion of the Stelling with earthy materials, and thus converting the Stellings or Wharfs to a certain extent into a solid quay. These erections have converted the line of water side into a succession of creeks, from which the descending force of the river, in ebb, has little effect in clearing away carrion and other matters drifted in by the flood-tide and drainage of the town, or the rotten salt fish, damaged vegetables, and rubbish of all kinds thrown by the merchants in great quantities over the Stellings. Over some of the Stellings, where the water is stagnant, the most offensive smells arise, and the white paint of the wooden houses is speedily blackened and reduced to metallic lead. The inner row of houses in Water Street is in general lower in level than the water-side row, being built within the embankment, on the original level of the country, and over what was the trench of the dam. This difference of level between the two sides is less apparent now than it was in 1837; for the trench is everywhere but in the Kingston District filled up, the streets are elevated, and the inner level has, in most cases, been artificially raised. At spring-tides, nevertheless, the river frequently rises above the Stellings, streams across the street, and carrying with it as rich a warp or alluvial deposit as that of the Humber, settles on the low ground of the inner row of houses.[1]

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[1] *Stellen*, to place—German or Hoch Deutsch. The same term is used in the Platte Deutsch.

Regent Street, the southern boundary of Robb's Town, has by the Vlissingen Canal a river frontage similar to that of Water Street.

Ships from North America and the West Indies, and those whose lay-days are few, generally lie alongside of some private Stelling or Wharf. The British merchant ships lie out at moorings in lines fifty, one hundred, or one hundred and fifty fathoms from the shore; and there discharge and load by the assistance of boats. The majority of the ships' crews are in this manner as completely isolated from each other as if each ship were in quarantine. The river is rapid, dangerous, and infested with sharks, and no communication can take place between the ships except by the boats. Of the amount of trade to this port some idea may be formed by a table given by Dr Blair of the number of seamen who have arrived in every month for twelve years since 1835; and as the arrivals and departures nearly balance each other, a constant number of 200 to represent the number remaining at the 31st December 1844, and added to the arrivals each month, will give a close approximation to the total number of seamen who form the river or harbour population. The arrivals, it may be observed, are pretty constant throughout the year, as Guiana, unlike the West India Islands, grows and manufactures sugar, the great export, in all seasons indiscriminately.

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[1] Warp is the black alluvial earth deposited by a river on its overflowed banks.

Monthly Arrival of Seamen in Demarara River.

Years.	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sep.	Oct.	Nov.	Dec.	Total.
1835	256	216	246	270	316	318	255	360	260	306	433	517	3753
1836	338	183	343	327	319	334	280	516	365	380	505	217	4107
1837	365	292	378	332	384	199	341	285	363	200	622	460	4216
1838	385	515	257	425	318	402	376	371	387	562	285	419	4702
1839	291	345	272	319	272	415	381	208	416	473	419	346	4157
1840	385	263	307	544	245	405	501	484	363	440	588	388	4863
1841	658	202	667	271	365	417	398	322	345	337	336	352	4670
1842	378	417	370	276	380	242	298	341	415	217	583	292	4209
1843	278	177	413	249	381	288	383	271	276	467	459	308	3950
1844	297	411	266	301	308	201	332	194	434	683	401	459	4137
1845	273	358	372	387	376	431	477	511	460	230	507	471	4853
1846	430	447	518	466	466	345	369	444	297	499	384	517	5182
	4334	3826	4404	4167	4120	3997	4391	4307	4379	4644	5522	4696	52799

The harbour for some time previous to the outbreak of Yellow Fever in 1837, had a high character for healthiness. But after the epidemic had continued for one or two years, seamen could scarcely be induced on any terms to ship for Demarara; and of these many stated that they had been grossly deceived;—not a few who were shipped as for Norway, finding themselves moored in the Demarara River.

The following Table shews the mortality among the seamen in the river and about Georgetown for twelve years.

Absolute Mortality amongst Seamen from the 1st January 1835 *to the* 31st *December* 1846, *on board Ship about Georgetown, Demarara, and in the Seamen's Hospital, inclusive.*

Year.	Jan.	Feb.	Mar.	Apr.	May.	June	July	Aug.	Sep.	Oct.	Nov.	Dec.	Total.
1835	1	...	...	1	...	...	...	...	...	...	...	...	2
1836	1	1	3	3	2	...	2	...	...	...	...	...	12
1837	...	1	1	1	7	39	45	29	37	44	23	30	257
1838	43	37	25	10	15	38	44	30	27	20	20	17	326
1839	7	6	4	3	4	2	4	9	22	23	49	24	157
1840	24	17	4	2	3	5	2	3	...	3	3	10	76
1841	16	17	6	1	2	2	5	19	43	11	13	18	153
1842	13	8	10	2	2	11	9	13	15	32	12	8	135
1843	3	...	...	1	...	1	1	2	2	1	2	8	21
1844	...	4	...	...	1	2	...	3	...	1	1	1	13
1845	1	...	...	1	1	1	2	1	...	1	3	...	11
1846	...	3	1	...	1	1	1	...	1	...	1	...	9

To the east and north-east of the Military Grounds, and on the front lands of Plantation Thomas and Kitty, there were many hundred acres of jungle, forming a well-sheltered swamp. To the north during 1837, there also was a naked marsh of about two hundred and fifty acres. During each high spring-tide, the sea covered the surface of the marsh. The soil was composed of the usual constituents of the fore-shores of British Guiana, namely clay, a fine sand known by the name of *Caddy*, and a mixture of light fat earth and vegetable matter known by the name of DRIFT MUD. The surface was jagged by a great number of half-rotten stems and roots, probably the vestiges of some previous cultivation, rising several feet above the level of the swamp. On its sea-margin was springing up a forest of young Corrudas (*Avicennia nitida* and *A. tomentosa*), and other trees which delight in a brackish humid soil, and require to have their roots and stems under water. The marsh was covered with tufts of coarse grass, the leaves of which sheltered myriads of insects, while throughout, burrowed innumerable crabs (*Cancer ruricola*, Linnæus; *Gecarcinus ruricola*, Latreille.) Fragments of drift wood, bones, dead spawn, dried mollusca, and small fish, left by the recession of the tide were profusely scattered over the surface. Near the public road were small gullies, communicating apparently with the jungle, lined with cryptogamic plants and containing frothy putrid-looking water. The editor here adds that the air-bells disengaged might be oxygen from the decomposition of carbonic acid by the Cryptogamia under the influence of light. Within the trenches aquatic larvæ and exuviæ abounded, and over them clouds of mosquitoes and sand-flies darkened the air.

In the neighbourhood of this spot were the Military Grounds; and in close proximity to leeward of the marsh stood the Military Hospital. It is said that almost every case admitted into this hospital, during the epidemic, no matter what was the ailment on admission, became Yellow Fever; and it ultimately was an object of such dread to the soldiers, that it was most difficult to persuade them when sick to enter it. Its position seems certainly to have been selected with singular judgment and great knowledge of sanitary principles.

The most important elements of weather in British Guiana are the wind and the rain.

As the inhabited sea-coast lies nearly NE. and SW., the trade-winds perflate the whole line. Hygienically speaking, there are only two winds, the land and the sea breeze. The sea-breeze is cooling and invigorating; the land-wind induces a feeling of chillness and discomfort, not explicable on mere meteorological characters, and in many persons a feeling of squeamishness. These land-winds are universally believed to be unwholesome. A low total velocity of the wind guage is an indication of their prevalence. The south-east land-wind at the end of the great rainy season, feels chilling in consequence of the evaporation from the surface of the body which its extreme dryness causes, while the sun is unclouded, and heats the air and the surface of the ground. This is unhealthy weather. Continued fevers are then prevalent.

The rainy season is the prevailing period of the land-winds, and during the night and morning of that season chiefly. There are two rainy seasons, one the long or great rainy season, beginning in the middle of April, and lasting till the end of July, with torrents in June, and showers in August; the other the short rainy season, and beginning in the latter end of November, and lasting till the end of January. During the little rainy season, the rains are light, and frequently from the north and east, accompanied by the refreshing sea-breeze.

It is supposed that the transition from one season to the other is the most sickly period of the year. This, however, can apply only to the transition at the autumnal equinox. When the rain clouds have disappeared, the sun is fervently hot, and the trade-winds have not yet been thoroughly established. The vernal equinox, and the ensuing two or three months, are the healthiest portions of the year. The inflammatory diathesis is most prevalent during the dry season; and the congestive and adynamic during the rainy season.

Extreme seasons modify not only the type of disease but the effects of treatment. In the height of the great rainy season of 1846 which followed nearly nine months of continued dry weather, the adynamic and congestive type of fever became so intense and so prevalent that the usual emetico-purgative saline mixture of the Colonial Hospital was found too depressing and violent for the patients, and its use had to be given up. When the rate of humidity is considerable, drastic purgatives should be avoided, and at such times mercurial preparations require to be given with caution, as they are liable to induce ptyalism. During such weather diminished thirst and increased secretion by the kidneys are observed; and if this weather continue long, total congestions, indicated by headaches, drowsiness, and sopor or coma, or watery alvine discharges, become complications among hospital patients. New rain, that is the first showers of the season, are liable to induce Bronchitis with sibilant *rhonchi*, Pleurisy, œdematous swelling of the lower extremities, and Diarrhœas. The

Diarrhœas are ascribed to the use of rain water from the roofs, impregnated with lead; though Dr Davy, the editor, thinks that its hurtful effects may be owing to the presence of infusoria.

The transition from the great rainy season is attended with Continued Fevers, marked frequently by typhoid symptoms. The quality of the rain presents a difference in regard to the comfort of the colonists; showers descending in large heavy drops, leaving the sky clear and blue, and render the air dry and pleasant. Those again which descend in small elongated drops may pour down the same amount of rain; but the air feels damp and foggy afterwards.

The coldest and hottest weather follow the position of the sun; the former being generally in February, and the latter in July and August.

The intense heat which might be expected to arise from the double path of the sun over the zenith between the months of March and September is mitigated by the canopy of thick clouds which then frequently covers the sky, and by the rains at the middle period of the year, as well as by the physical circumstances of the sea-coast.

The temperature is of course tropical. On the 21st day of June 1843 it was 78·2° in the morning between six and seven o'clock; 80° at eight and fifteen minutes; 83° at noon; 84° from one to two o'clock; 79° till near midnight; and during the nocturnal hours till six next morning, 78°.

The population of British Guiana is very miscellaneous. Almost every nation of Europe is there represented. Creoles from the West India Islands, and North Americans are numerous. The dark races are composed of Coolies from Madras and Calcutta, Negroes from the slave-dealing nations of Africa, and the aboriginal American Indian. Pure specimens of these races, and native descendants of them, are numerous. But there is a large class of mixed races, chiefly between the white and the negro, in various proportions. The migratory population in the merchant shipping is composed chiefly of white races of the various nations of Europe, and the States and Colonies of North America, with a small proportion of the Negro race, principally employed as cooks on board the vessels.

The population and the cultivation of the colony is confined chiefly to the sea-coasts and mouths of the rivers. A few wood-cutters inhabit the creeks and banks of the rivers in the interior. The pure Indians shun the towns, unless when they come for the purpose of barter, or in search of temporary employment.

The most important classification of the population in a physiological and medical point of view is made by dividing the inhabitants into the newly-arrived, and the long resident; the former including all the inhabitants within one or two years of their arrival; the latter, the Creoles or natives, and the strangers who have been settled in the colony longer than two years. To the first class may be referred clerks in the merchant stores, overseers on plantations, sailors in the river-shipping, and the white troops.

The people are in general well fed, and procure in abundance the means of subsistence. The standard and favourite vegetable food of the colonists is the plantain, and to this the constant accompaniment is the salted cod-fish from North America. But, besides these, hogs and poultry are reared and consumed in considerable quantity; salted meat, as beef, pork, and hams, are used freely; and fish and game are easily procured. Salted provisions, especially pork, are very commonly used. Indeed, the fondness for salted and highly-seasoned food is general.

The water in general use is rain-water, collected from the roofs of houses in vats, tanks, and cisterns. It is slightly impregnated with sea-salt. Artesian well water, which is chalybeate, is used by the poor during droughts. Creek or river water taken up above the tide is used, and said to be much liked, especially by seamen.

Cocoa and coffee, especially the latter, are much and freely used.

The Diseases from which the colonists are entirely free are Contagious or Infectious Fevers, excepting the Exanthemata, Calculus, Diabetes, Rabies. Those from which they are nearly exempt, are Tubercle, Dyspepsia, Aneurism, and Malignant Tumours.

The Exanthemata and Hooping Cough are mild and of rare occurrence.

Albuminuria, when it occurs, is a curable symptom. Patients with Chronic Bronchitis rapidly recover after arrival in the colony.

The air is so mild that the knee and other joints are opened without hesitation, if the symptoms require the operation for the escape of inflammatory or sero-albuminous products. The subcutaneous operation is unnecessary. Compound Fractures, which in other countries would be looked upon as hopeless, generally terminate favourably. Flesh wounds heal by the first intention.

On the other hand, Intermittent and Remittent Fevers prevail so extensively, and with such severity, among the inhabitants not of African origin, that the countries of Demarara and Essequibo require about three thousand ounces of quinine for annual use, or nearly one scruple for each individual. Gout and Rheumatism are not uncommon; and Tetanic Irritation is easily set up, particularly in the Negro race. Inflammation of the external lymphatics and the lymphatic glands, too, according to Dr Blair, is common; and to its neglect he ascribes, in imitation of Dr Hendy, Elephantiasis, or the Swelled Leg; (*Buknemia.*) Painter's Colic, though rarer and less hurtful, since the disuse, as the author alleges, of drugged claret, is still frequently met with. Disease of the liver, though seen, is not common.

Pneumonia and Pleurisy occur chiefly in consequence of reckless exposure, and in anæmious subjects. Dysentery, though formerly acute and epidemic (endemic?) is now chiefly seen as a sequel of Intermittent Fever; and though accompanied by ulcerations of the large intestine, has more the symptoms of Diarrhœa than those of Dysentery.

Fever and *Fĕbricula* (*Ephemera?*) as a consequence of gastric and encephalic Irritation and Inflammation, frequently arise, in consequence of errors in diet, insolation, and suppressed perspiration, particularly among newly-arrived persons who are intemperate, and indulge in laborious exercises.

The great endemics, however, of British Guiana are Miasmatic Fever and Ulcer.

The basis of the Miasmatic Fever is Intermittent or Ague. Quartans are met with; more frequently Tertians; but the ordinary type among the highly susceptible and exposed is the Quotidian. Unusual susceptibility in the constitution, an unusual intensity in the pathogenetic influence from meteorological or other causes, or an inflammatory complication, will shorten the intermissions more considerably, until the disease becomes Remittent, and even Continued. The danger is least when the intermission is longest, and inversely. These fevers differ altogether, according to Dr Blair, from what is called Yellow Fever. But he allows that the intense forms of Continuous Intermittent have often been confounded with Yellow Fever, and the treatment which has been successful in such instances has been called successful treatment of Yellow Fever. The correctness of this remark is confirmed by the editor, Dr Davy; and we believe it is in accordance with the experience of all those who have most correctly and carefully observed the phenomena of Yellow Fever epidemics. At the same time, it must be remarked, that Dr John Hunter, Dr Pinckard, Dr Jackson, Dr Fergusson, and many others bear testimony to the fact, that in certain, if not in all seasons, cases of Yellow Fever were observed to pass into Tertian Intermittents; and Remittents and Yellow Fevers were observed to coexist during the same epidemic.

The Miasmatic or Malarial Intermittent Fever is so powerful in British Guiana, that the Yellow Fever epidemic could not supersede it, and the physicians of Georgetown found it sometimes engrafting itself on the weakened convalescents from Yellow Fever; in a few cases it seemed to dispute the possession of the victim; in some it modified the progress of the symptoms. Among 6395 admissions in the Colonial Hospital for the twelve months from June 1846 till June 1847, 2938 were cases of Intermittent and Remittent Fever, and and 1873 were persons labouring under ulcers. The prevalence of Intermittent Fever, nevertheless, the author thinks, would be underrated, by supposing its influence limited to the production of 2938 among 6395 hospital cases; because many in the balance, for instance cases of *Anœmia*, Dropsy, *Splenalgia*, and Dysentery, were sequelæ of the neglected instances of the same distemper.

Dr Blair infers, from observing the diseases of the thousands of immigrant settlers of late years arrived in the colony, and those who present themselves for treatment at the Colonial Hospital, that two species of Malaria or Miasma are endemic in British Guiana. One is an *Anœmiating* Malaria, the other a Septic Malaria. The former, he adds, pervades the colony, so that, a ruddy cheek is nowhere to be found after one year's residence; but it affects the seashore occupants in the smallest degree. Its intensity increases as we advance into the interior, up the narrow creeks and muddy rivers. There the complexion becomes perfectly blanched and sallow or dingy, even without any attack of Intermittent Fever. The spleen becomes enlarged, the cellular tissue infiltrated, and dyspnœa, and palpitations supervene in consequence of mere diminution of the red particles of the blood, and, the author adds, thinness of that fluid.

An anæmiated immigrant Portuguese, residing eight or ten miles up the river may get an ulcer; but though the granulations are pale and the progress of cure is slow, the sore heals in a kindly manner. No virus seems to be present. Another immigrant Portuguese of equal residence, but with an appearance still tolerably healthy will shew himself at the Hospital with a gangrenous phagedæma, speedily destroying the soft and solid parts; and if from the irrecoverable condition of the limb, amputation be deemed necessary, gangrene will probably attack the stump within twenty-four hours afterwards. Both patients have been subject to Intermittent Fever; but the former might at once have been pronounced a labourer from some of the river estates, and the latter most likely from the islands of Wakenaam, Leguan, or some of the coast estates. The idea has often occurred to Dr Blair that this Septic modification of the miasmatic poison, which shews its operation in the bleeding gangrenous ulcer, may be some imperfect development of the Yellow Fever Poison.

It is very probable that this idea rests on good foundation. At all events, nothing is better ascertained than the fact, that in all miasmatic countries and districts, the miasmatic or malarial poison has a twofold operation; one chronic, another acute; one producing peculiar bad effects in the blood, in the liver, the spleen, and the whole alimentary canal and vascular system; and the other giving rise to fevers, Intermittent, Remittent, and Continuous. This at least was observed long ago by Lionel Chalmers in Carolina; was very fully observed and its effects described by Foderé in the Maritime Alps in 1823; was remarked by James Lind as to Holland and other countries; was seen in the West India Islands by various observers; was remarked by Pinckard in Demarara; and is matter of daily observation in the lake and miasmatic districts of the United States. Abundance of evidence upon this question has been adduced in various volumes of the Annales d'Hygiene. But long before the date of this instructive Journal, Foderé had directed particular attention to the existence and operation of the chronic form of miasmastic poisoning, as it is most extensively observed in the district called the Sologne in France, and as it is seen at *Frejus*, Saintes Maries, Aigues-Mortes, and other places in the Maritime Alps Department, and that of the mouths of the Rhone. This writer gives the following as a faithful description of the physical appearance of the inhabitants of the Department of Ain, one of these miasmatic districts.

A pale livid complexion, a dull sunk eye, the eyelids swelled and œdematous, numerous wrinkles furrowing the face at an age when soft sounded forms only are observed in other provinces, the shoulders narrow, the chest confined, a long neck, a shrill feeble voice, the skin at all times either dry or bathed in debilitating perspirations, a slow and painful mode of walking, and all the symptoms of distress in the lungs; old age at thirty years; deformity and decrepitude at forty or fifty, if life be prolonged so far; such is the inhabitant of Bassé-Bresse and Dombe, of that extensive marsh interrupted only with some detached fields and some gloomy forests. Born amidst the physical causes of insalubrity, he is early made to feel their fatal influence. To the enjoyment and spirits of infancy, to the hilarity and adventure of youth, he is almost constantly a stranger. A valetudinary state occupies the place of health. He sleeps amidst sufferings, and awakes only to feel pain.[1]

In his other accounts of the chronic effects of miasma upon the health of the inhabitants of such districts, he notices not only the enlargement of the liver and spleen, but the frequent prevalence among the female part of the population of *Amenorrhœa*, *Leucorrhœa*, Sterility, and Abortion, the short valetudinarian lives, and deaths frequent at periods comparatively early. All these circumstances are observed to take place without the occurrence of open fever, intermittent, remittent, or continuous. It seems as if the air daily inspired from the hour of birth, united with the blood and contaminated that fluid by its poisonous principles, preventing not only the due development of the frame and its constituent organs, but deranging the organs themselves, and inducing such a thorough change, that the system is no longer able to react with the vigour and energy necessary to induce fever.

The same phenomena are witnessed in inhabitants of the lake districts of the United States, and those located along the banks of the Mississippi, the Missouri, the Red River, the Arkansas, and similar situations. It is indeed well known that this chronic poisoning is one of the great impediments to the success and progress of new settlers in these regions.

The author mentions not, whether all these morbid states are presented by the long residents of the river banks of Demarara. But it can scarcely be doubted, that if they are not known at present, they will eventually be known. We are informed that in the southern conterminous territory belonging to France, at Cayenne and its neighbourhood, these effects are perfectly perceptible, generally diffused, and most destructive.

In British Guiana, according to the author, this chronic miasmatism shews itself mostly in the formation of bad ulcers, —intractable, phagedenic, and hæmorrhagic. These ulcers are as to character various. Among many of the Negroes they are large, with indurated edges, produce not the slightest inconvenience except in the trouble of dressing them, are called chronic and constitutional; and to heal them no attempt is made. From healing such ulcers indeed bad consequences have occasionally resulted. Among the immigrants, especially the Portuguese and the Coolies, the ulcer is generally sloughy, phagedenic, or corroding; bleeding, and sometimes proceeds to perfect sphakelus without surrounding or previous erythema. The predisposing and exciting causes in the production of these ulcers are the malaria of the colony and the small wounds of insects and serrated grasses. The ulcers are seated chiefly in the lower extremities; but sometimes the fingers and sometimes the lips and cheeks are affected.

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[1] Medécine Legale, par F. E. Foderé, D.M., Tome Cinquieme, § 1107. Paris, 1813.

All these evils, nevertheless, appear not equally to assail all. The endemic influences are confined chiefly to the labouring classes, and among these to the White inhabitants, and those who either are not sufficiently fed, or who are debauched and intemperate, who are much exposed to the weather and night air, and who use no clothing for their feet and legs. Care, good treatment and management protect even the White man. The Whites do not deteriorate in the climate; and, according to the author, good specimens, both of White and Creole inhabitants are seen; and extreme old age is not unfrequent.

It must be allowed, the author acknowledges, that there is a greater amount of sickness in British Guiana than in England. But it is much more temporary; and among the middle and upper classes he represents the expectation of life to be as good as in any part of Europe. Of course Yellow Fever epidemics make sad havoc among new immigrants from Europe; and these may be expected to be more frequent and more intense as population increases. On the other hand, the climate and air of the colony seem to be highly salutary to those who suffer from the winter cold at home, to the narrow-chested, the weak-lunged, and the scrofulous.

From the account of the soil, surface, and climate, here given, it will not be matter of astonishment that the country of British Guiana is productive of Yellow Fever. Close upon the north of the equator, it is manifestly within that zone of the terraqueous globe, in which yellow fever is known to be endemial.

Stabroeck, the old Dutch name for Georgetown was founded about the year 1774; and there is a tradition, that about nineteen years after this date, namely, in 1793, an epidemic of yellow fever prevailed with some severity, though not for long duration. This is not unlikely, as it was the noted year when the disease appeared in Granada, Barbadoes, and other West India islands, extending eventually to New York, and became a subject of controversy between Dr Chisholm and a number of other writers.

Another epidemic is said to have appeared in Georgetown in the year 1819, was exceedingly virulent for about nine months, and then gradually stopped. This epidemic commenced at the stores of Mr Simpson and Mr Kernon in America Street. Of one hundred white persons, merchants and clerks, inhabiting America Street, and the neighbouring parts of Water Street alone, thirty-three died. This is very much the usual rate of mortality, one in three in all similar situations. Georgetown was then not peopled by any means so densely as it has since been, and as it is at present. But from the plans of old Stabroeck and the modern Georgetown, as given by Dr Blair, it seems not only not wonderful, that remittent fever should be indigenous, and yellow fever a frequent visitant, but extraordinary that it is ever absent.

On Sunday morning, the 8th day of April 1837, Dr Alleyne and Dr Blair were conversing on the health of Georgetown. Dr Alleyne stated that he had treated some cases that terminated fatally at the time, when the severity of the symptoms had ceased, and when he looked upon the patient as convalescent. Dr Blair then remarked that he had at that time a case which he believed to be yellow fever, and both physicians went to see the patient. On the way Dr Alleyne suggested that they should go to the house of Mr Rankin, druggist, at the corner of Robb's Stelling, Water Street, where he had a case similar to those referred to as having terminated fatally. This, when seen, was allowed to be a case of Yellow Fever, and the patient, by name Rainey, died next morning.

Dr Blair and Dr Alleyne were joined by Dr Hutson, all of whom examined the case. "The symptoms," says Dr Blair, "we considered to be certainly those of yellow fever, as far as our book knowledge enabled us to decide."

The name of the patient of Dr Blair was Inch, one of two brothers, young Irishmen, who had arrived in the colony two or three months previously, and acted for a short time as plantation overseers. One or two weeks before the commencement of his illness, Inch and his brother had hired a ground floor of a house in Water Street, in the inner side, the most depressed of the street, and which was in no way raised above the ground, and commenced keeping there a small shop, in which they sold porter, plantains, salt fish, tobacco, and similar miscellaneous wares. All these articles, at the time of the attendance of Dr Blair, were in great confusion, few being on shelves, the rest strewed without order on the floor. The room was small, 8 feet by 20, being merely the centre gateway of the house portioned off, and not one-third of the ground floor of the building. The rest of the ground floor was occupied as a store for paints and oils by a house painter, who lived in the second story of the house. Inch lived in the same small room which formed his shop, and slept there in a hammock which he unslung during the day. The atmosphere of the place was bad enough, between the exhalations of rotten salt fish, tobacco, damp earth, and dirt. For two or three days, he was attended in this place. But on the day before Drs Alleyne and Hutson saw him with Dr Blair, the latter had got him removed to a side building in the same yard, on the second floor, the roof of which, however, was so low, that it was impossible to stand erect in it. But the place was clean, and not hot. On the 8th day of April, when Inch was seen by these gentlemen, he was yellow, black blood oozing from his gums, and from a sore in the leg. In the evening he had black vomit. Yet from these symptoms he recovered, and lived ten years afterwards, when he died at Leguan on the 1st February 1847.

The family of the house painter were never attacked. But the brother of Inch, who had been residing about 150 yards further north on the same side of the street, and who attended on the sick brother night and day, sickened during the convalescence of the first, and died on the fifth day with black vomit, with yellow skin and purple patches after death. These young men were muscular, fair, of sober habits, and between twenty-one and twenty-five years old.

The next case was that of Mr Coey, on the same side of Water Street, and about 150 yards south of Inch's shop; or between that shop and Rankin's. This patient recovered.

The previous cases which Dr Alleyne had attended, were a few yards further south, at the corner of Regent Street and Water Street, the inner side, and in Regent Street, on the water side, about midway between Water Street and the Vlissingen Sluice. Such, during that month of the year reputed the most healthy, was the beginning of an epidemic disease, of which the oldest practitioner in the town had no previous personal knowledge, and which for years spread death among the new settlers, sailors, and soldiers, till Demarara became a name no better than New Orleans or Vera Cruz.

After the disease had thus commenced its appearance at the spots mentioned, it in no long time shewed itself in various other points of the colony, in Essequibo, New Amsterdam, and other parts in Berbice. Its course is thus described.

"The epidemic of 1837, which thus first appeared in Regent Street and Water Street, the centre of which space is Robb's Stelling, soon spread itself along the length of the city. Water Street, or its immediate vicinity, was always the portion chiefly affected, although a few straggling cases were to be met with in the back parts of the town, and one case (that of the late Mrs D.) even as far back as at the manager's house of Plantation Vlissingen.

"*After* the appearance of the disease in the city it was noticed in the mercantile shipping, as will be illustrated by reference to the table of mortality among the sailors. Cases then made their appearance in the Essequibo coasts and islands, and in New Amsterdam, Berbice. The troops suffered most in 1839, but what retarding circumstance (perhaps that of unsuitable subjects) acted in their case, I have no means of ascertaining.

"The first cases in Berbice were those of the Reverends Messrs Wray and Howes (fatal) in May 1837; the Reverend Mr Ketley (recovered), 16th June; a young man recently from Demarara (recovered) clerk of Messrs Foderingham; Mr Parish, schoolmaster of the London Missionary Society on 4th July (fatal). After these five cases the disease began to appear among the sailors, and continued to rage among them till November, when it suddenly disappeared. The disease also broke out in the Canje garrison about the same time as among the shipping. Few cases occurred in the rural districts, and those chiefly after a visit to New Amsterdam. In four out of the first five cases, the patients had been three days previous to the attack in Georgetown, Demarara. The disease in Berbice did not spread *immediately* after the death of any of the fatal cases; and there was a long interval between the last case on shore and the first among the seamen.

"Many cases of fatal yellow fever occurred at the sand-hills, about thirty-five miles up the Demarara river, the first elevated land, composed of little else than snowy-white sand, which, however, bears a variety of forest trees, the chief of which have long since been cut down. These cases, however, only occurred in vessels which, *after waiting* in vain for cargo *in Georgetown*, proceeded up to the sand-hills for ballast. During the epidemic, many ships, knowing the difficulty of procuring freight of sugar after the slave emancipation, and finding the hard woods of the colony in demand for ship-building in Europe, proceeded into the interior by the Essequibo river, and remained there for weeks cutting timber on the sand-hills of that region, during which time the crews were exposed to all the vicissitudes of weather, using tremendous exertions in felling and hauling the heavy logs, and often working with half the body immersed in water, and the whole force of the sun acting upon them, while engaged in the laborious occupation of heaving the logs aboard; yet the yellow fever never visited them there, and they remained healthy, while the ships' crews in the mouth of the Demarara river were losing one-third or one-half of their men.

"On full inquiry it would seem that, although the mouth of the De-

> marara river was the chief seat of the pathogenic influence of the epidemic, as well as furnishing, most numerously, susceptible subjects, yet the morbific cause was not confined to that locality; but extended more or less along the whole *coast* of the colony. The focus of disease in this locality seemed to be the Mud Lots of Georgetown and their immediate neighbourhood. Many strange and true predictions of attacks of the epidemic from known exposure occur among my notes;—many neglecting warnings followed by fatal penalties. Some merchants at last retreated from their dwellings above their stores in Water Street when any unseasoned member of their family rejoined it from Europe. The further retired in town, the safer the residence was felt to be. Projects for building a river wall and filling up the Mud Lots were openly discussed, and £100 premium was awarded by the Town Council for the best plan. Ships were moored and unmoored to get rid of unhealthy positions in the river-stream, and men held their breath in passing the Stellings.
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> "Till 1842 I had charge of the plantation hospitals of 'Lapenitence,' 'Ruimveld,' 'Rome and Houston,' on which were many Madeira immigrants, and I found that in proportion as these immigrants approached town or coast-ward in their locations the yellow fever predominated, and as they receded the intermittent fever asserted supremacy. 'Lapenitence' suffered most from *yellow* fever, and 'Rome and Houston' least."

Though in this manner the sickly state of the colony shewed its effects in many different points, yet Dr Blair ascertained, by careful observations, two facts; one, that the spot where the disease made its first appearance, namely, Water Street, retained its original character for the severity and high mortality, the general rate being 24·15 per cent.; and that though the poison was diluted, yet the distemper was most prevalent at the mouth of the river, the moorings most sought after by shipmasters, on account of the advantage of the free open breeze.

The author gives a table shewing the attacks and mortality among the crews of the shipping in Demarara River.

It appears that there were 270 vessels moored in the river during the time at when the Epidemic prevailed; and these might have an average crew of twelve each vessel, that is, about 3240 seamen. It appears that 943 cases of fever took place, of which 614 are marked under the head of Gravior; that is the severest form of Yellow Fever; all the residue being Intermittent, Remittent, *Simplex* and *Mitior*. The deaths amounted to 186 persons over all. But at certain stations on the river the deaths appear to have been more numerous than at others. Thus, at one point, Holmes to Robbs, the deaths are stated to have been so high as 37·31 per cent., while on two other points they were between 27 and 28 per cent., and on one not more than 25 per cent.

"Formerly Dutch ships traded regularly to this port. The two last arrivals from Holland moored in the Demarara river when the epidemic had burst over and was ravaging the shipping. They lay higher up the river than any ship enumerated in the above table, and what happened to them was conformable to that which would be expected at present. On board these ships seven persons were attacked with intermittent fever after the ships lay eight weeks, and the cases came on almost simultaneously. One man caught yellow fever and died, his illness having occasioned no alarm till he was almost moribund. The captain of one (Captain Matzin) sickened, but recovered. Thus two cases only occurred on board of these ships high up the river, although several months in moorings during the worst time of the pestilence."

Nothing very satisfactory appears to have been ascertained regarding the comparative danger and insecurity of different stations on the river, by reason of their proximity to and remoteness from the banks. While some ships lost half their crew, others almost within stonethrow, and possibly to windward, had no sickness, or were losing no men. But positions did not remain permanently healthy, or the reverse; and the difference in mortality was not ascribable to any difference in treatment or the reverse. Some of the cases indeed might be regarded, till after the establishment of the seamen's hospital, as untreated, in consequence of the inadequate attendance of the medical practitioners, who were overworked, and the total want of anything like proper nursing, or of those comforts which fever cases require.

From the month of April 1837, all through May, June, and July, the disease continued to prevail both in the town and among the shipping; and as the treatment on board was considered inefficient and inadequate, in August 1837 an attempt was made to establish an hospital for the sick merchant seamen, by the Colonial Government hiring a house on Urquhart's Stelling or landing-place. The situation seems not to have been judiciously chosen; but probably there was little liberty of selection. The seamen very generally refused to leave their ships.

About the end of 1837 the disease appears to have been rather less prevalent, though those that took place were as intense as ever; and in the beginning of 1838 the number of cases was much diminished;—a reduction which coincided in time with a similar diminution,—Dr Blair calls it a sudden cessation,—of the epidemic at Berbice. The temporary hospital was closed at the end of December 1837, the last two cases having been discharged on the 26th.

Again, however, as the season advanced and warm weather appeared, the disease increased, and continued to prevail among the shipping and in the town among those recently arrived. In December 1838, it was found requisite to open a seaman's hospital for the admission of all the sick of the mercantile shipping. After this time, accurate returns of the attacks and the deaths were given, so that the movements of the disease can be more easily and with greater certainty traced than previously.

It appears that the distemper continued during 1839, 1840, 1841, 1842, 1843, 1844, and 1845, with different degrees of intensity and severity. The disease is distinguished into three forms,—*Simplex*, *Mitior*, and *Gravior*, according to the severity and complexity of the symptoms. No cases of Simple Yellow Fever are noted during 1839 and 1840; and this mild form of the disease appears not to have been recognized till 1841, though Dr Blair expresses the opinion that it was present during the entire epidemic. The attacks and deaths are exhibited during the years specified by the following table.

	Febris Flava Mitior.	Febris Flava Gravior.	Total.	Deaths.	Ratio.
1839	...	234	234	103	44·01
1840	113	311	424	39	12·54
1841	14	666	680	123	20·00
1842	168	345	513	110	31·88
1843	55	69	124	17	24·64
1844	61	15	76	10	66·66
1845	18	2	20	2	100·00

From this table it results that the disease was most extensively prevalent in the years 1840, 1841, and 1842; that its mortality was very great in 1839, 1841, 1842, 1844, and 1845; that in 1843 its prevalence had undergone a decided diminution, and its mortality was also abated; and that this diminution continued during the subsequent years of 1844, 1845, and 1846. It is added, that in the great rainy season of 1845, the disease entirely ceased, superseded by slight but very general Influenza.

In considering the causes of this epidemic, Dr Blair allows that the question as to the circumstances from which the disease arose, is in this quite as difficult as in other instances.

On one point only he speaks with confidence. The disease was not contagious, and shewed none of the characters of a contagious disorder.

On Predisposing, Exciting, Determining, and Auxiliary Causes, the following remarks are made.

"The grand *predisposing* cause of an attack of the epidemic was the state of constitution, induced by a previous and recent residence in a cold climate. The grand *exciting* cause, during several years, was exposure to the influence of certain localities, the chief of which was the *embouchure* of the Demarara River. Georgetown, and the shipping, being situated within the malarious locality, and their population containing the largest proportion of northern blood, the disease began there first and continued there last. At the beginning of the epidemic, and till after the lull at the end of 1837, Europeans of many years' residence, and some of the black and coloured population, and Indians suffered from the disease. Among the old European residents, or white Creoles of the West Indies, however, when it occurred it rarely ended fatally, although the type was of the gravior form. After 1838, the epidemic became almost peculiar to new comers from cold climates.

"Intemperance was occasionally a predisposing cause by recklessness of exposure; but abstinence, as shewn on board of the American *teetotal ships*, was no protection. In fact *delirium tremens* was not an unfavourable complication of the disease.

"Sometimes the *determining* cause seemed of the slightest description,—the shock on the stomach by an ice cream or glass of iced punch, or the indigestion of an unripe orange, would occasionally set the train of symptoms in motion. It seemed at one time as if those resident in the infected districts circulated the poison habitually through their system; that old residents had in an eminent degree the power of eliminating it and keeping its presence latent,—had a tolerance of it; but that new comers, and particularly those of florid complexion and rigid fibre, were constantly, in reference to the presence of the virus, in a state of *tottering equilibrium*; so that in them the slightest unfavourable impulse to the balance,—the lowering of the vital powers by fatigue, the suppression of any of the depurating secretions, a shock to either of the nervous centres, or the depressing emotions,—were sufficient to excite the latent poison.

"There was no external sign by which the more or less perfect saturation of the system could be detected during health. But in those cases,—and they were chiefly among new comers,—where the immediate attack was referable to a slight determining cause, the disease always exhibited its utmost violence. An instance of the determining effects of the depressing emotions is well exhibited in the case of Mr Rankin, an old colonist, the druggist, close by Robb's

> Stelling, referred to in the first chapter. Mr R. had been in good circumstances up to a short period before his death in 1842, and although living in the midst of the worst district of Water Street, he retained robust and uninterrupted health. About 1842 he became involved in the general mercantile distress then prevalent in Georgetown; in fact he was utterly ruined. Moreover, immediately before his illness, he became particularly depressed by what he supposed to be treachery on the part of one of his friends. The malaria then produced its deadly results on him. He died of black vomit, after having passed unscathed through the pestilence of the previous years. Even fretting and temporary chagrin have been the determining cause of a fatal invasion: an instance, the notes of which are now before me, is in the case of Dr Leitch, late surgeon to the "Arabian," immigrant transport. On Saturday morning the 22d September, 1843, the proprietor of a plantation on Wakenam engaged to call for Dr Leitch, to carry him for a few days to his estate. The gentleman neither came at eleven o'clock, the hour appointed, nor sent any message of explanation. Dr Leitch, all prepared, waited hour after hour on the quarter deck (under awning) till evening, and was much fretted by the disappointment. That night he got the yellow fever, and died on the sixth day of his illness with black vomit. In the same page of my notes with Dr Leitch I find a case of death from yellow fever, the determining cause of which also was distress of mind. It was a mulatto, of the name of Felix Theome, a native of Martinique, whose goods had been distrained in Berbice, by a Commissary of Taxation, on the plea of their being smuggled. As was to be expected in a constitution of the tropical regions, his system struggled hard against the strong tendency to death. He died on the twelfth day of his illness, and four days after black vomit had ceased. The mental emotions had not only the effect of developing the disease in the susceptible, and those who had been exposed to the morbific localities, but also played an important part in the procession of the symptoms and on the result."—Pp. 58–62.

A minute and apparently faithful account of the nekroscopic appearances derived from the analysis of ninety-seven fatal cases of yellow fever, carefully inspected after death, is given in chapter third of the second division. All that we can give of this part of the volume is the following general summary.

> "The *Blood* in the dead subjects was almost invariably abnormally thin and black.
>
> "The second anatomical characteristic of the disease was the hæmorrhagic lesion of the mucous and submucous surface of the alimentary canal, and the peculiar excretions therein contained. The cardiac end of the stomach was the chief site of the inflammatory or

congestive hyperæmia. The œsophagus, particularly at its gastric extremity, was next in frequency affected. The affection of the œsophagus had, however, some peculiarities, and chiefly in being attended with a peeling or erosion of the epithelial coat in longitudinal stripes. This erosion might occur without hyperæmia, and *vice versa.*

"The erosion was always more or less marked as vomiting had been more or less severe during life. The intestines followed the œsophagus in frequency of lesion. Sometimes the congestion or ecchymosis of the mucous and submucous surface of the alimentary canal was so intense and extensive as to give them a sphacelated appearance, and an appearance of gangrene to the whole intestinal peritoneum, on opening the wall of the abdomen.

"The villi were the chief seats of engorgement, sometimes to perfect blackness. The hyperæmia had sometimes a stellated appearance, sometimes dotted or blurred or striated, sometimes arborescent, and generally of a rusty red colour; frequently inflammation, congestion, and ecchymosis were so blended as to defy discrimination. The hyperæmiated or eroded surfaces were generally clothed with viscid mucus; sometimes the stomach was of a uniform deep claret colour, as if by the imbibition of imperfectly formed black vomit, and giving its peritoneal coat a rosy hue. Sometimes the general red discoloration of an apparantly inflamed piece of stomach or intestine could be scraped off without injuring the mucous tissue. On such a piece being slightly washed of its mucosities and held up to the light, the appearance resolved itself to a mere film of blood, adherent apparently by the undeveloped epithelial matter. The ecchymosis was occasionally in the mucous, frequently in the submucous, and only once in the subperitoneal tissue. The unusual condition of the solitary and aggregate glands seemed due entirely to surrounding and interstitial congestion and ecchymosis. At the ilio-cœcal valve the hyperæmia was thickly punctuated, like a close-shaven black beard.

"Submucous arborization was of general occurrence in the large vessels. In one case it was in six-inch length patches in the jejunum, and of three feet in the ileum, accompanied with extensive ecchymosis. Sometimes the capillaries (particularly of the œsophagus) were developed as in chronic ophthalmia, but of a dark colour. The ulcerations occeasionally shewed a foul sloughy base, but in general they were merely peelings or erosions of unusual depth, and they were in logitudinal stripes. Hyperæmia frequently reddened and elevated their edges. Their surface was frequently formed of the submucous tissue and meshes of dead capillaries. The sources of the black vomit could always be traced to hæmorrhagic spots generally in the stomach, but if not there in the œsophagus or duodenum. The presence of black vomit in the stomach after death was the rule, its absence the exception. It was generally found in considerable quantities, so that on turning over the cadaver it would gush profusely out of the mouth and nares. Black vomit seemed merely an ulterior effect of the hæmorrhagic condition of the blood, mucous membrane, and capillaries; and although sometimes absent, its antecedent lesion was always present in a pure case of death from yellow fever. An early and fatal complication might destroy the patient before the disease was matured, and then, of course, many of the anatomical signs would be absent. If the patient died of secondary symptoms, then many of the anatomical signs were absent, for they had passed away. Bloody black vomit was found in death after the third stage of the disease had shewn a tendency to convalescence."—(Pp. 100–102.)

We have refrained, in the foregoing remarks, from saying anything on the question of Contagion, as a cause or means of propagating Yellow Fever; not because we think the question of no moment, but because we believe that anything that can be said is altogether useless. The rational and observant practitioners in the West Indies are all convinced that the disease derives its origin from local causes, from peculiar atmospheric and terrestrial conditions, from certain states of the atmosphere operating upon the surface of the ground. To these, therefore, it is unnecessary to address any remarks or any arguments. The confident defenders of contagion are not to be convinced by any facts or arguments which might be adduced. To them all historical evidence, all testimony from personal observers, are entirely useless. One division of this section of the profession talk of pestilence, Pestilential Fever, Bulam Fever, *Typhus Icterodes*, African Typhus, and all sorts of newly-coined terms. They should, in consistency, admit a distinct Yellow Fever for every spot on the globe, in which that disease has been known to appear. With such advocates it is quite loss of time to reason. It is better to have, upon the causes of diseases, no opinion at all, to acknowledge that the subject is beyond the grasp of the human faculties, than to adopt and defend opinions, which can be just by no other way than by disregarding all facts, however well established.

If additional evidence were required on the question relating to the origin of Yellow Fever and the mode of its propagation;—if further confirmation were necessary of the justice of the doctrine, that Yellow Fever is not an imported disease, but arises within certain geographical limits, and under the influence of certain terrestrial and atmospheric conditions, the present volume may be justly mentioned as furnishing that evidence, and one which stands in the same rank with the writings of Dr John Hunter, Dr Jackson, Dr Bancroft, Dr John Wilson, M. Chervin, M. Thomas, Mr Amiel, and Dr Gillkrest. To any one who reads the description of the physical and local peculiarities of Georgetown, it cannot appear wonderful, it must be quite natural, that it should be a favourite and prolific source of Yellow Fever. The disease ceased indeed in 1845, as most epidemic visitations, after it had destroyed all the susceptible part of the population. But it has since that time returned with little less force and not diminished mortality. This is not all. The inhabitants of Georgetown must be prepared for future visitations of this malady, always becoming more violent and more destructive, perhaps more frequent, as the town increases in population, and as Europeans and Americans resort to their shores in the pursuit of commerce and trade. It will be impossible to prevent these visitations, just as it is impossible to prevent them at New Orleans and Vera Cruz. But something may be done to render them less violent and less destructive. The river should be embanked with substantial stone quays; the dwelling-houses and all places for the accommodation of inhabitants and visitors, especially sleeping apartments, ought to be removed from the banks and close vicinity of the river. If to remove dwelling-houses from this locality be impracticable, all sleeping apartments should be in the stories above the basement one; and the more they are elevated above the ground, the safer will they be. In such a situation indeed no one should pass the night on a ground apartment. The streets should be raised above their present level, perfectly paved and provided with good underground drains. All wooden erections should either be prohibited or restricted to the smallest possible amount. The surface should be more perfectly drained; and the ditches and canals should be reduced in number as much as possible.

Regarding the vessels moored in the river, it is not unlikely that in not a few of these vessels the disease was generated, under the atmospheric and climatic influence within the ships themselves. An excellent system, in such a view, would be a naval police, with power to inspect vessels and enforce cleanliness.

Of Treatment we have said nothing. No method almost seemed effectual. But calomel and quinine are the remedies which were most frequently used.

ART. IV.—1. *Icones Septem* CAROLI AUGUSTI BOCKII *Descriptionem Nervorum Spinalium Illustrantes.* Lipsiæ, 1828. Folio.

Seven Figures, illustrating the course of the Spinal Nerves. By CHARLES AUGUSTUS BOCK. Leipzig, 1828.

2. *Observations on the Structure and Functions of the Spinal Chord.* By R. D. GRAINGER, Lecturer on Anatomy and Physiology. London, 1837. 8vo. Pp. 159.

3. *Samuel Thomas Von Soemmerring, Vom Baue des Menschicher Korpers.* Neue umgearbeitete und vervollstandigte Original-ausgabe. Besorgt Von W. TH. BISCHOFF, J. HENLE, G. HUSCHKE, F. W. THEILE, G. VALENTIN, J. VOGEL, und R. WAGNER. Vierter Band.

Samuel Thomas Von Soemmerring Hirn und Nervenlehre. Umgearbeitete. Von G. VALENTIN. Mit Einer Kupfertafeln. Leipzig, 1841. 8vo. Seite 772.

Samuel Thomas Von Soemmerring on the Structure of the Human Body. A New Edition. Edited by W. TH. BISCHOFF, J. HENLE, G. HUSCHKE, F. W. THEILE, G. VALENTIN, J. VOGEL, and RUDOLPH WAGNER. Fourth Volume.

Samuel Thomas Von Soemmerring on the Anatomy of the Brain and Nerves. By G. VALENTIN. Leipzig, 1841. Pp. 772.

4. *The Descriptive and Physiological Anatomy of the Brain, Spinal Chord, and Ganglions, and of their Coverings.* Adapted for the Use of Students. By ROBERT BENTLEY TODD, M.D., F.R.S., Fellow of the Royal College of Physicians, Physician to King's College Hospital, and Professor of Physiology in King's College, London. London, 1845. 12mo. Pp. 284.

5. *The Human Brain: its Structure, Physiology, and Diseases. With a Description of the Typical Forms of Brain in the Animal Kingdom.* By SAMUEL SOLLY, F.R.S., Senior Assistant Surgeon to St Thomas's Hospital, and Lecturer on Clinical Surgery, &c. &c. Second Edition; with numerous Wood Engravings. London, 1847. 8vo. Pp. 628.

6. *Plates of the Brain, in explanation of the Physical Faculties of the Nervous System.* By JOSEPH SWAN. London, 1853. 4to. Plates XXII.

These publications, illustrating the Anatomy and Physiology of the Nervous System, are in different modes, and on different grounds entitled to attention. The latter five are treatises of very considerable importance in the history and literature of the Anatomy of the Nervous System.

I. The Seven Figures by Charles Augustus Bock are confined to the Spinal Nerves,—that is, the nerves which may be directed to and arising from the spinal cord. The author, however, has not included those important nervous branches which are connected on the one hand with the spinal, and on the other with the internal vital organs. The representations relate chiefly to the superficial nerves; those especially of Sensation, and some of those which are referred to the Reflex Sensations by Dr Marshall Hall.

II. The volume by Mr Grainger, on the Structure and Functions of the Spinal Chord, is well deserving of attention, and presents an instructive account of the departments of Neurography and Neurology, to which the author confines his attention.

The work consists of Seven Chapters and an Appendix.

After a short review of the opinions entertained by different authorities on the Functions of the Spinal Chord, in which the doctrines of Robert Whytt, Sir Charles Bell, Dr Marshall Hall, and Johann Muller, chiefly are examined, Mr Grainger considers, in Chapter Second, the properties of the Gray and the White Matter of the Nervous System. Mr Grainger adverts to the well-known fact of the great vascularity of the gray matter, and the large quantity of blood which it receives, when compared with the white fibrous matter. He remarks the great increase of the gray matter in proportion to the amount of nervous energy. He notices the large proportion of gray matter in the spinal chord at birth, when the cerebral convolutions are imperfectly developed,—shewing that the spinal functions are in full activity at a time when the intellectual faculties, sensation, and volition, are almost entirely inactive, if not wanting. Lastly, Mr Grainger observes that the gray matter is only met with in those parts of the Nervous System which are known to be the seat of power,—that is, the brain, spinal chord, and ganglions; and that it is wanting in those parts which have not the faculty of giving origin to power,—namely, the nerves,—which are merely conducting organs.

The circumstances now enumerated indeed, lead Mr Grainger to the conclusion, that the white fibres are merely conductors of power; and where these fibres preponderate, as in the nerves, the power of exciting sensation or motion does not exist.

The conclusion at which the author arrives in this Chapter is, that the phenomena displayed in the cerebro-spinal system are dependent on the gray matter; and that it is probable, that the same substance which composes so large a proportion of the Ganglia, performs an equally important function in the system of the Great Sympathetic.

The Third Chapter is devoted to the Anatomy, properly so called, of the Spinal Chord. A correct account is given of the origin and mode of origin of the Nerves, both from the Brain, the *Medulla oblongata*, and the Spinal Marrow itself. Mr Grainger is favourable to the distinction of Incident and Reflex Nerves, as introduced and made the subject of commentary and illustration by Dr Marshall Hall.

The Fourth Chapter is expressly devoted to the Physiology of the Spinal Chord. Mr Grainger here enters very fully into the doctrines of the Reflex Function, and gives an interesting view of the facts.

In the Fifth Chapter the author gives a general summary of the results upon the whole subject.

In the Sixth Chapter he gives a theory of the operation and influence of the Great Sympathetic Nerve; and in the Appendix is presented a classification of the parts composing the Nervous System according to Physiological principles.

This treatise may be justly recommended, as presenting in a short compass an intelligible view of the doctrines most generally recognised upon the Functions of the Spinal Chord. It will not be expected that we should give any opinion upon the correctness of these doctrines. It is enough that we say that the author states with clearness his propositions and deductions, and shews that they are supported by a reasonable amount of probable evidence. Physiology, however, is a progressive science. What is true to-day and at the present time, may be controverted to-morrow, and in the course of the ensuing year may be entirely disproved. But it must not be forgotten, that treatises like the present form integrant and necessary parts of the history and progress of Physiological Science.

III. The volume of G. Valentin is an elaborate and comprehensive treatise on the General and Descriptive Anatomy of the Nervous System.

It perhaps may be proper to remind our readers, that the work of Samuel Thomas Soemmerring was originally published in the German language between the years 1791 and 1796; and that a Latin translation of this by the author himself was published at Frankfort-upon-the-Maine, between the years 1794 and 1800. A second edition of the work appeared in the German language in 1800 and 1801. Beyond the limits of Germany the work was known chiefly by the Latin edition. It was always distinguished for methodical arrangement, for clearness, sufficient amplitude and great precision in description, with great brevity and condensation; and, as such, it maintained its reputation as one of the most serviceable Treatises on Anatomy which the physician or surgeon could study. This work was deficient, however, in not giving the anatomical description of the eye, the ear, and the reproductive organs in the two sexes. The two first-mentioned defects the author supplied in his ABBILDUNGEN DES MENSCHLICHEN AUGES, folio, Frankfort, 1801; and ABBILDUNGEN DES MENSCHLICHEN HOERORGANES, folio, Frankfort, 1806; while his Treatise on the Base of the Brain gave some useful details on the minute descriptive anatomy of that organ.

This treatise by Soemmerring had, for a long series of years, maintained undiminished its early character among the anatomists of Germany, and various other continental countries, notwithstanding many able competitors; and one of the testimonies not the least equivocal to its merits, is, that almost all other systems were formed very much after its example; some acknowledging the fact, others being contented with copying without acknowledgment.

As years however advanced, and as anatomists resorted to the frequent and general use of the microscope, a new world was as it were opened to their eyes, and minute structure required to be explained in greater detail. Several of the principal anatomical teachers in the German states were quite aware of the great merits of the Treatise of Soemmerring; but were also aware that much additional information was requisite in order to render it a correct and ample system of anatomical knowledge, towards the middle of the nineteenth century. In consequence of these feelings it seemed most expedient to those authorities, to republish in 1840 the work of Soemmerring, enlarged and improved by suitable additions; and, accordingly, between the years 1841 and 1844 the work was published at Leipzig, by Bischoff, Henle, Huschke, Theile, Valentin, Vogel, and Rudolph Wagner, in eight volumes octavo, with a large amount of new and accurate information.

Of this New System, the fourth volume, on the Anatomy of the Brain and Nerves, is the work of G. Valentin. And the author states, that, with one exception, not very considerable, the whole descriptive part of the volume has been prepared from repeated personal researches, and verified by preparations, which in the instance of difficult and doubtful parts of anatomy, were expressly made for the purpose. The exception to which the author alludes, is, that in the posterior and some of the anterior branches of the nerves of the spinal marrow he has followed the work of the late Charles Augustus Bock, the same anatomist whose engravings of the spinal nerves have already been mentioned.

The endeavour of the author, he informs us, was to present an index or description rather of human Neurology, as complete as possible; and this duty he has performed in the following manner.

First comes a General Part, occupying one hundred and thirty-two pages, devoted to the General Anatomy of the Nervous System; or, as the author expresses it, the General Morphology, the Philosophical Anatomy, and the Chemistry of the Nervous System in Vertebrated Animals, and particularly in Man.

The General Morphology and the Philosophical Anatomy occupies the largest portion of this division. In it the author describes the general characters and minute structure of the brain and nerves with great care, and in an elaborate manner, in one hundred and twelve paragraphs. The information is derived from all the best authorities as well as from personal researches. The order observed is as nearly as possible the following.

First, the author after distinguishing the Nervous System into the Peripheral portion or the Ramified Chords, and the Central portion or the Brain and Spinal Marrow, gives a short account of the Primitive Nervous Fibres, the Nervous *Corpuscula*, the Neurilema, the Nervous Content, the Varicose or Moniliform Nervous Fibres, the Gangliar Globules, the Central Primitive Nerve Fibres, the Central Nucleated Globules, Ramification of the Nerves, Anastomosis, §§ 42, 43; the Ganglia, § 47; Plexuses, §§ 53, 54; the forms of the Courses of the Nerves, §§ 62, 63; Terminations of the Nerves in Organs, § 64; Enlargement of Nerves in their Course, § 70; the Size or Thickness of Nerves, § 71; Mode of Origin of the Spinal Nerves and the Cerebral Nerves, §§ 73, 75; the Sympathetic Nerve, § 83, in its Cervical, Cephalic, and Sacral Portions, § 89; and then the Central Portion, namely, the Brain and Spinal Chord.

The Chemistry occupies but a small space, being discussed in three short paragraphs.

The Special Part, or the Descriptive Anatomy of the Nervous System, occupies the rest of the volume, amounting to six hundred and two pages.

This may be distinguished into Five subdivisions; one devoted to the Descriptive Anatomy of the Brain and its coverings and divisions (pp. 153, 226); a Second devoted to the Spinal Marrow (pp. 226, 235); and the measurements of its different parts; a Third part employed in explaining the disposition of white and gray cerebral matter, fibrous matter, and globular matter, in the several divisions of the Brain, the *Medulla Oblongata*, and the Spinal Marrow (pp. 235–288); a Fourth, to the Origin of the Nerves (pp. 288–292); and the Fifth and last, to the description of the course, distribution, and termination of all the Nervous Chords in the Human Body (pp. 292–754).

It is no part of our plan or intention to enter into any examination of this elaborate volume. Since it has appeared we have repeatedly read and studied various portions of it with attention; and we can say, that the author has spared no pains to give correct views of the state of knowledge of the anatomy of the nervous system, up to the date of publication. He has evidently been not a little embarrassed by the conflicting testimony of different observers as to points in microscopical anatomy; and there are several, perhaps many, instances in which it is evident, that so great is the discordance, that a long time must elapse before very consistent views can be communicated. With all these drawbacks, nevertheless, the work is one both of interest and instruction.

From the objections now stated, the purely descriptive portions are altogether free; and especially those relating to the description of the course, distribution, and termination of the nerves, may be recommended as presenting most faithful representations of the anatomy of this portion of the system.

The whole volume is a valuable repository of facts on the Anatomical Structure of the Brain and Nerves.

Of this volume, with the others, a French translation was published at Paris in 1843.

IV. The Descriptive and Physiological Anatomy of the Brain, Spinal Chord, and Ganglions, by Dr Robert Bentley Todd, appeared originally in the Cyclopædia of Anatomy and Physiology, under the title of Nervous Centres. It was republished in 1845 with several additions and some alterations suggested by further experience.

In its present shape the work consists of Twelve Chapters, in which the Anatomy of the Brain, Spinal Chord and Nerves is explained in the following manner.

After stating, in sixteen Aphorisms, certain definitions and propositions regarding the Nervous System in general, the author treats, in the First Chapter of the Ganglions and their Coverings; the Cerebral Membranes; the Cerebro-Spinal Fluid, the Pacchionian Bodies, and the Denticulate Ligament.

In Chapter Second the structure of the Nervous Centres and of the Ganglions is considered.

Chapter Third is devoted chiefly to the elucidation of the Anatomy of the Spinal Chord, and the origin of the Spinal Nerves.

In Chapter Fourth, the Anatomy of the Brain and its parts and regions is fully explained.

In Chapter Fifth the Anatomy of the Medulla Oblongata, its divisions, and the Nerves which issue from it, is explained.

In Chapter Sixth, the author treats of the Mesocephalon, the Protuberance, and the Quadrigeminous Bodies.

In Chapter Seventh the subjects considered are the Cerebellum, the Vermiform Processes, and the *Corpus Dentatum* of the Cerebellum.

In the Eighth Chapter Dr Todd gives a general view of the structure of the Hemispheres of the Brain and the Convolutions, compares them with those of the inferior animals, describes the Insula of Reil, the situation and structure of the Hippocampi, of the Corpora striata, and of the Thalami, the situation and structure of the Geniculate Bodies, of the Mammillary Eminences, the *Tuber Cinereum*, the Pituitary Body, and the Membrane of the Ventricles.

In the Tenth Chapter, the Bloodvessels of the Brain, and the Circulation through them are considered, and some observations illustrating the nature of Delirium from *Anœmia* are made.

The Tenth Chapter is devoted to the Anatomical History of the Nerves,—their origin, course, and distribution.

In the Eleventh Chapter Dr Todd treats of the microscopical anatomy of the Brain and Spinal Chord.

The Twelfth and last Chapter is of a physiological and mixed character. The author submits to his readers his hypothesis of what is called Action of the Brain. He treats of the mechanism by which the action of the Brain may be supposed to operate; and makes some observations on the action of the Cerebellum.

Whatever opinion may be entertained on the value of these speculative inferences, it must be allowed that the anatomical history is given with as much clearness as the subject admits, and with great accuracy.

The volume is illustrated by woodcuts, many of which shew the minute microscopical structure of different parts of the Brain, the Spinal Chord, the Nerves, and the Ganglia, according to the observations of the most skilful observers.

V. The first edition of the work of Mr Solly was published in the year 1836; and in April 1837, we had occasion to announce its appearance and speak of its merits in the forty-seventh volume of this Journal. These merits were such that we were enabled to recommend the work in strong terms, as one presenting an instructive view of the Anatomy of the Cerebral portion of the Nervous System.[1]

The present edition, which appeared eleven years after the first, is a greatly enlarged and improved form of the work.

The whole treatise is divided into Nine Parts or Principal Divisions, in which the author treats successively of the following subjects.

The First Part is devoted to the topic of Structural Anatomy, in which the author treats of the Physical, Chemical, and Microscopical Characters of NEURINE, the name which he applies to the peculiar substance in which he believes that the powers of the Brain and Nerves reside. He disapproves of the term Nervous Matter. There are two kinds of Neurine,—one gray or ash-coloured and pulpy in appearance; the other pearly-white in colour, and fibrous in appearance. The first being found to consist of nucleated cells or vesicles, he denominates *Vesicular Neurine*. The second,—the white and fibrous,—being found to consist of tubes, he denominates *Tubular Neurine*. A third variety of this matter, which has been long known as the constituent of the Sympathetic, he allows to be called Filamentous or Gelatinous Neurine,—also in consequence of its microscopical characters.

The gray vesicular neurine is, according to Mr Solly, the source of power, the agent of intelligence. The white or tubular neurine is the conductor of power, and performs the functions ascribed to the excito-motor nerves.

Part Second is devoted to the extensive subject of the Comparative Anatomy of the Brain and Nerves. In this division the author takes an extensive view of the different forms in which the nervous system appears in the classes and tribes of animals; beginning with the Aspondylous class, the Worms, the Radiated animals, the Articulate, the different tribes of Molluscous animals, and tracing the structures upwards through the Insects, the Fishes, the Reptiles, the Birds, to the Mammalia, and the Human Race.

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[1] Edin. Med. and Surg. Journal, vol. forty-seventh, p. 477, April 1837.

In this Part the author treats not only of the Brain, but of the Spinal Chord and the Great Sympathetic, the only parts of the nervous system found in some of these classes of animals; the whole aspondylous class presenting no spinal chord, properly so called, but a corresponding central portion of nervous system, the gangliated thread, not inclosed within a bony case.

The Third Part of the work is devoted to the Anatomical History of the protective apparatus of the Human Brain; the Cranium and Spinal Column, the *Dura mater*, *Pia mater*, and other membranes.

In Part Fourth the author gives the weight of the Brain and its parts, according to the best authorities, and concludes with some physiological deductions.

In Part Fifth, Mr Solly enters upon the Anatomy of the Brain, properly so called, treats of the configuration of the Brain under the two heads of the *Convoluted* and the *Figurale* surfaces, describes the eminences by which the latter is distinguished, and the ventricles.

The Sixth Part is devoted to the Dissection of the Human Brain and Spinal Cord, and the most eligible methods of investigating and exhibiting the minute structures, and the mutual relations, of the component parts. The author here introduces his ideas regarding the distribution and arrangement of the two varieties of Neurine; and endeavours to apply them in explaining the organs and seats of sensation; locomotion, the excito-motory function; and he is at great pains to present correct views of the structure of that most complex and important portion, the Annular Protuberance and the Medulla oblongata.

After describing all these parts, he gives a short view of their general character and mutual relation in the following summary.

In the first place, we have an extensive surface of Cineritious Neurine, the hemispherical Ganglion, speaking merely of one side of the brain, which in the higher orders of animals is convoluted or folded in a peculiar manner.

In apposition to the whole of the vesicular Neurine of this Ganglion, there are tubular fibres which radiate through it, and are encrusted by its nucleated cells.

These fibres are disposed in four different ways. First, some of them commencing from the convolutions of the anterior, middle, and posterior lobes, pass through the Corpora Striata, and, forming the inferior layers of the *Crus cerebri* or limb of the Brain, pass through the *Pons Varolii*, or Protuberance, so as to form the anterior columns of the chord, as previously described; The *Motor Tract*. Secondly, Other fibres commencing in the Nerves of sensation and after passing through the *Pons Varolii*, and emerging from the substance of the Thalamus, terminate on the same Neurine that gave origin to the last. This is the *Sensory Tract*. Thirdly, Other fibres passing from one side of the brain to the other, and in opposition to the internal surface of all the convolutions, are those fibres which, collected into a mass, form between the hemispheres that wide bridge, as Mr Solly calls it, the *great transverse Commissure*, or *Corpus callosum*. Fourthly, and lastly, in contact with all the convolutions are the fibres of the superior and inferior longitudinal commissures, which connecting together those convolutions which are situate on the same side of the median line, or different portions of the same hemispherical ganglion, so far differ from the transverse commissure, which connects those situated on opposite sides, or the two distinct but corresponding ganglia.

The first and second set of fibres, which radiate from the external surface of the two large ganglia of the anterior and posterior columns, as from a common centre, forming, however, in their radiation only half a circle, were designated by Gall and Spurzheim the *Diverging Fibres*. The third set of fibres, which converge towards the centre of the brain, the transverse commissural, were distinguished as the *Converging fibres* by the same authors.

The above descriptions demonstrate that the Enkephalon or Brain of the human subject is not a large solid mass of matter, in the interior of which are cavities scooped, as it were, out of its substance, but that it really consists of ganglia or collections of cineritious neurine, placed on each side of the mesial line. Some of these ganglia are the appropriate ganglia of the nerves of sensation; for instance, the Olfactory Ganglia, the Optic Ganglia or *Tubercula Quadrigemina*, the Auditory Ganglia or Posterior Pyramidal Bodies, the Pneumogastric Ganglia or Restiform Ganglia, the Olivary Bodies or Lingual Ganglia; the others being the Motory and Sensory Ganglia, as the *Corpora striata*, and *Thalami nervorum opticorum*. The hemispherical ganglia, again, that they might present the greatest possible extent of surface, are folded up into innumerable plaits, and thus cover and surround every other ganglion within the cranium, so that on first removing the skull-cap nothing can be seen but the convoluted surface of these extensive ganglia.

Some observations on the most eligible method of studying the Anatomy of the Brain, and the Relations of its parts, conclude the division.

Part Seventh contains the Anatomical History of the Cerebral Nerves, the minute Analogy of the Decussation of the Optic *Chiasma*, and the Theory of Vision.

Part Eighth contains the Description of the Cerebral Blood-vessels, and the peculiarities of the Cerebral Circulation.

In the Ninth Part is given an account of the Development and Formation of the Brain, from the best authorities.

In the Tenth Part Mr Solly treats of the Physiology of the Cerebro-Spinal Axis, and subjoins some observations on Phrenology, in the principles of which he thinks there may be a portion of truth.

The Eleventh Part, which is extensive, and occupies the concluding portion of the volume, from page 373 to the end, is devoted to a subject at once of very great importance, but not less of difficulty. In this part Mr Solly treats of the diseased states referable to the Brain, either as connected with changes in its circulation, or as connected with changes in structure. These morbid states are distinguished into four general orders: I. Anæmic Affections; II. Hyperæmic Affections; III. Convulsive Affections; IV. Organic Affections.

Under the First Head are considered *Delirium Tremens*, Hysteria, Anæmic Coma, and the Hydrenkephaloid Disease; Softening; Fatuity, and Mania.

Under the Second Head are examined Inflammation of various kinds and in various parts, the Cortical Substance, the White Matter, the Base, the Cerebellum, the Hemispherical Ganglia; Meningitis, Arachnitis, Mania as an effect of Inflammatory Irritation; Concussion and its Effects; Fractures of the Skull; Otitis and its Effects; Apoplexy of different kinds and forms; and the treatment required for each disorder.

Under the Third Head are treated Concussion as the effects of External Injury; Epilepsy, Centric and Centripetal.

Under the Fourth Head are considered Morbid Growths from Neurine; from the *Dura Mater*; Malignant or Cancerous Growths; Hæmatoma or the Blood-Tumour; Hypertrophy; followed by some observations on the weight of the Brain when diseased.

In this Division of his Treatise Mr Solly has been at great pains to present correct views of the Pathology and Treatment of Cerebral Diseases; and the work must be allowed, though much of a compilation, to be one of a most instructive and serviceable character.

VI. The Plates of the Brain by Mr Joseph Swan, the most recent of these works, form a valuable addition to the stock of knowledge upon the Anatomy of the Nervous System.

Mr Swan has been so long known as an industrious and intelligent inquirer into the structure of the nervous system, that any work proceeding from him ought to possess a high value indeed. It is now thirty years since we had occasion, in the pages of this Journal, to direct attention to the labours of Mr Swan upon certain of the diseases of the nerves. He has since that period given a new Anatomical History, with elaborate Illustrations of the ramified divisions of the Nervous System; a valuable adjunct to the researches of the physiologist and the physician.

The present publication may be regarded as completing the series, since it presents these illustrative explanations of the structure and arrangement of the Central Portion of the Nervous System, without which all reasonings on the supposed functions must be limited, vague and liable to not few errors.

In a clearly written Introduction, Mr Swan gives a general view of the Structure of the Brain, the relative proportions and dispositions of white and gray cerebral matter, and the leading characters which distinguish the brains of different animals and different classes of animals. He makes use of the peculiarities in the structure of the *Medulla oblongata* and its parts, in order to explain what he denominates the Physical Faculties of the Nervous System. Like most recent writers on these subjects, Mr Swan distinguishes, in the Medulla oblongata and the parts connected with it, a Sensitive Tract, a Voluntary Tract, and an Involuntary Tract.

1. The Sensitive Tract extends from the Posterior Pyramidal Bodies in Man, to the floor or basis of the Fourth Ventricle. It is continued upwards at the base of the Quadrigeminous Bodies, and forming a thick cord, assumes some resemblance to a ganglion just before the Geniculate Bodies; and behind the posterior commissure it passes outwardly across the posterior margin of the Thalamus, beneath the Optic and posteriorly to the Involuntary Tract.

2. Each Anterior Pyramidal Body represents a Voluntary Tract, and after forming combinations in the annular protuberance, with layers of the inferior *Crus* of the cerebellum, becomes a *Crus* or limb of the Brain. On entering the hemisphere, it appears as if its larger portion were immediately,

separated into innumerable threads, radiating freely among the gray matter in the internal oval receptacle, which forms the base of the Striated Body. The anterior and larger portion of it spreads out beneath the gray matter of the surface of the Striated Body. The second portion of the *Crus*, which is smaller than the first, and the third, which is the smallest, after forming combinations with the exterior portion of Gray Matter, is inserted into a perpendicular stratum of White Matter, which forms the partition between the internal and external oval receptacles.

3. Each half of the Involuntary Tract in Man begins at the ring on the under side of the Thalamus, and extends backwards through the upper and posterior surface of the corresponding *Crus* or limb of the Brain. Each half is separated from the other half by the sides of the Passage from the Third to the Fourth Ventricle. (*Iter a Tertio ad Quartum Ventriculum*).

Near the lower end of the Annular Protuberance (*Pons Varolii*) the Involuntary Tract becomes expanded laterally in giving origin to one half of the larger portion of the Fifth Pair or Trifacial Nerve. At the lower margin of the Annular Protuberance, the origin of the Auditory Nerve forms a slight connection with its posterior surface. The Tract then becomes expanded in a broad thick heart-shaped Ganglion, having the base uppermost; and after the removal of the Anterior Pyramidal, the Olivary, and the Restiforn Bodies, the Glosso-pharyngeal Nerve and Par Vagum, are seen firmly attached to and deriving from it their principal origin. At the bottom of the oblong *Medulla*, the apex of this heart-shaped ganglion terminates in the continuation or prolongation of the tract, which is extended throughout the Spinal Chord at the bottom of the deep anterior fissure, and becomes combined with both the anterior and the posterior quarters of the Chord.

In the first dissections, says Mr Swan, the Involuntary Tract was traced by taking out its internal parts, composed of pouches filled with interwoven gray and white matter, and leaving the cylinders which formed its internal boundaries. This condition is represented in several of the plates.

The Involuntary Tract is the source of a peculiar perceptive and irritable power manifested in the organs to which its nerves are distributed. Any moderate impulse is confined to the excited organ and its nerves. A more permanent impression gradually and almost imperceptibly modifies the activity of the muscles subservient to it, either through the nerves connected with it, or through the convolutions, until a state agreeable to its perceptions is restored.

The small number of Nerves given off by the Involuntary Tract perform very extensive and complex functions. These nerves are part of the Fifth Pair, or Trifacial, or Tergeminal; the Glosso-pharyngeal; the Pneumogastric, *Nervus Vagus*, or Tenth Pair; and some filaments which are connected with the Great Sympathetic.

Through the half of the large portion of the Fifth Nerve or Trifacial, originating from the Involuntary Tract, it associates the nose, the mouth, and their appendages, and allows the muscles to be called into action, either alone, or in concert with other nerves and organs under its influence, as in taking food, and especially in sucking and in the movements of respiration.

Through the Glosso-pharyngeal Nerve the back of the tongue and fauces are exerted in the act of swallowing; through the *Nervus Vagus*, in concert with the Glosso-pharyngeal Nerve, the powers of the Pharynx, the Œsophagus, and the Stomach are exercised.

But the Involuntary Tract is prolonged and extended into the Spinal Chord; and by this extension, and its probable connection with some of the roots of the Spinal Nerves, as well as through several plexuses formed between its Nervous filaments and those of the Sympathetic, the muscles of the chest and abdomen are brought into action in consonance with the parts receiving its nerves, in the acts of sneezing, coughing, speaking, singing, and various modifications of the voice; also in ruminating, vomiting, and other acts of evacuation of parts of the alimentary canal.

The Nerves of the Involuntary Tract, according to Mr Swan, have a much higher quality, from their direct communication with the Sensorium, than the Sympathetic, from that effected only through other Nerves. Its nerves and those of the Sympathetic possess each their peculiar properties and powers. Their intimate combinations, however, denote an approximation of function; and especially as together they form the sources of energy in the processes of Digestion, Purification of Nourishment, and Circulation and Reproduction. The Nerves of the Involuntary Tract communicate action to organs in the process of taking food, in digestion, in respiration; while the Sympathetic, in connection with the *Nervus Vagus*, modifies the circulation and the secretions under the variations of those processes. The several Plexuses formed between the Sympathetic, the Glosso-pharyngeal, and the Pneumogastric, combine the Viscera for the more passive consentaneous functions of the structures, composing, for instance, those of circulation, secretion, and perception. The associated arteries for swall~~owing, breathing, and vo~~miting are produced more particularly by the Glosso-pharyngeal Nerve and *Par Vagum*, through the Involuntary Tract and Spinal Chord and Nerves; while the actions for evacuation of the rectum, the uterus, the bladder, take place through the combinations between the hypogastric Plexus and Sacral Nerves, more particularly through the inferior extremity of the Involuntary Tract.

To shew the structure above described, and concurring in the action as now represented, Mr Swan has given many distinct views of the parts of the Brain in Man and several Animals,—both the superficial parts of the two surfaces, and the source and direction of the fibrous structures in the substance of the organ. The Plates are in number twenty-two. It is unnecessary to describe the objects represented in them. But it may be mentioned, that in the third, fourth, fifth, and sixth, are given views of the several Tracts now mentioned. In the twelfth Plate are exhibited the Involuntary and the Sensitive Tracts, with their connections with the Optic Nerve, the Fifth Pair, the Auditory Nerve, the Glosso-pharyngeal, and the *Par Vagum*; and in the thirteenth, the fourteenth, and fifteenth Plates, the anatomical relations of the same objects are still more fully and distinctly represented.

Plate sixteenth is a view of the Brain of the Horse; Plates seventeenth and eighteenth, of various parts of that of the Ox, and the oblong medulla of the same animal; Plate nineteenth gives views of various parts of the Brain of the Horse, and that of the Pig and Sheep; Plate twentieth gives the base of the Brain of the Ox, views of the Tracts, and of the Spinal Chord in the same animal; the twenty-first Plate is chiefly occupied in giving views of the Involuntary Tract in the Ox and Horse; and the twenty-second Plate presents views of the structure of the Medulla Oblongata of the Horse, the Ox, and the Sheep.

Whoever is conversant with the history of Anatomy, and the labours of those who have studied the department of Neurography, will allow that this publication is the most valuable and original contribution to the knowledge of the Structure and Functions of the Brain that has been made during the last half-century. Whoever wishes to form correct ideas of the internal structure of the Brain, and the mutual relation of its component parts, will find it quite indispensable to study these Plates with attention.

PART III.

MEDICAL INTELLIGENCE.

I.—MEDICAL PATHOLOGY.

1. *On Granular and Fatty Degeneration of the Voluntary Muscles.* By EDWARD MERYON, M.D., &c. Medico-Chirurgical Transactions, vol. XXXV., p. 73. London, 1852.

1. The Hon. Geo. P— was born in March 1834, a fine and apparently healthy child. He was large and heavy, but not in the least degree disproportionate; he passed through the period of early infancy without more disturbance of health than is usually manifested during the process of teething, but walked late, a circumstance which was attributed to his size. He walked heavily, however, and never with any appearance of healthy elasticity. He seemed to be a dead weight, having no power of assisting or jumping from the ground. This incapacity of springing was noticed by his father even before the heavy walk excited the slightest anxiety. He gradually improved in walking; but his movements were always of an inactive kind up to the age of six or seven, at which period he could walk a mile without resting.

His condition was wellnigh stationary until he was eight years old; if any change occurred, he rather lost ground than gained it. He ascended stairs with the greatest difficulty, by holding the balusters and dragging the leg which was farthest from them after him. This latter difficulty increased more rapidly than that of walking on a level surface. He was always disposed to trip, and could never make the slightest effort to recover himself. Between seven and eight he had hooping-cough, which neither affected his general health, nor did it produce any change in his power of walking. At nine he was placed under the care of a surgeon (Mr T—), who supposed that there was deficient elongation of the spinal cord (such were his words), and consequently put him on a stretching-board. It was at first thought that he was slightly improved by this plan of treatment, for he could raise his foot from the ground, assisted by his hands, somewhat higher than he could before; but his general health began to suffer, and he was then seen by Sir Benjamin Brodie and Mr Lawrence, when he was eleven years old. This was in March 1845. At this period he could not walk a step;—scarcely, indeed, could he stand;—and, at the suggestion of Mr Lawrence, he was placed under the care of Mr Tamplin, who considered that much of the failure of power might be dependent on muscular retraction; and he divided the tendo-Achilles of each leg and the tendons of four other muscles. About five weeks after the operation he walked, with the mechanical support which was applied to the feet and legs, but chiefly, as Mr Tamplin informed Dr Meryon, by the flexors and extensors of the hip-joint, and once, without irons he walked across a room unsupported. After that, however, he became gradually worse, losing by degrees the little power of walking which he had gained, and eventually of standing.

When Dr Meryon was consulted in November 1848, the power of the muscles of the upper extremities was diminishing also, notwithstanding that the muscular mass of the body and limbs did not appear to have diminished, but, on the contrary, he had grown well and had gained flesh. At this period he was taking, by the advice of Sir B. Brodie and Dr Bright, the Liquor Hydrargyri Bichloridi in Tinctura Cinchonæ Composita; and, supposing that the symptoms might depend on a strumous constitution and deposition of tuberculous matter either in the spinal cord or its investing membranes, which has been sometimes observed in children, the same combination of medicines was continued at intervals for nearly eighteen months, during which time he was generally living on the coast. But, in spite of every care, he gradually lost power, and in December 1850, whilst staying at Bournmouth, he was seized with what appeared to be a mild attack of fever, accompanied with nausea, giddiness, and a constant and profuse secretion of mucus from the trachea and larynx (such was the report of Mr Elgie, who attended him) but with no mucous rattle. There was an apparent paralysis of the pharyngeal muscles, repugnance to food, but constant thirst, a cool skin, with a pulse at 120, the breath having a cold, sickly odour; the urine loaded; the intellect remaining perfectly clear until the 19th. On the 20th, at four p.m., he died. On the receipt of a telegraphic despatch, Dr Meryon proceeded on Saturday, the 21st of December, accompanied by Mr Tatum, to Bournmouth, where they examined the body twenty-two hours after death.

The body generally presented an emaciated appearance, more especially the lower extremities; the chest was compressed and flattened in front, and there was a great lateral curvature of the spine. The knees and hip-joints were rigidly fixed in a bent position.

On examining the head, the dura mater was found a little more firmly adherent to the cranium than usual; the veins of the surface of the brain and those of the velum, as well as the sinuses, were gorged with blood, the sinuses containing a very loose coagulum. There was about an ounce of serum in the ventricles. With these slight exceptions, the brain was perfectly healthy.

The veins and sinuses of the spinal canal were turgid with blood, more particularly in the dorsal region; but the spinal cord itself and the membranes were perfectly sound and natural in appearance. About from three to four ounces of serum existed in the spinal canal.

The thorax and abdomen were next opened, and their contents carefully examined. The heart was healthy, both as regards size and firmness, and its colour was also of a dark, healthy red; the relative thickness of its walls natural, and the arterial system generally free from any morbid appearance. The lungs were perfectly healthy; and the whole of the abdominal viscera; the solar and lumbar plexuses of nerves, all appeared quite natural and healthy.

It was thought, both by Mr Tatum and Dr Meryon, that the venous congestion and serous exudation of the brain and spinal column might have been, and most probably were, the result of the disturbance which immediately preceded death.

The chief structural change existed in the system of voluntary muscles, which were throughout the entire body atrophied, soft, and almost bloodless; and, although the muscular fibres *appeared* to exist, yet were they not of the deep red colour seen in the healthy and natural state.

When the muscular tissue was examined under the microscope, the striped elementary primitive fibres were found to be completely destroyed, the sarcous element being diffused, and in many places converted into oil globules and granular matter, whilst the sarcolemma or tunic of the elementary fibre was broken down and destroyed.

The colour of the voluntary muscles generally was of an ochrey red; but, as the colour always varies according to the degree of development and activity, that physical character appeared of minor importance. It is well known, for instance, that the pectoral muscles of birds which do not fly are white, whilst the same muscles in birds of flight are red; and in the Teal, immediately after migration, they are intensely red. In carnivorous animals, again, the muscular mass is of a deeper red than in herbivorous animals.

A second son, the Hon G. Wr. P—, four years younger than the former, exhibited precisely the same physical condition and the same symptoms as did his brother at the same age; or if any difference existed, it was in the more rapid progress of the disease in the second son. In May 1847, when nearly nine years of age, he walked from Bruton Street to Westminster Bridge, but in November 1848, he could neither walk nor stand, and in 1850 his arms were fast losing power.

A third son, the Hon. Wm. P—, was born in 1845, a well-developed and healthy-looking child like the two former, but like them he always sat heavily on his nurse's arms. When able to walk he could never make the slightest spring, and in June 1850, when in his fifth year, he was much in the same condition as were his brothers at that age. When placed on the stairs and told to go up, he would hold on by the balusters, raise one foot to the step above, and drag the other after; the right foot being apparently dragged with most difficulty.

In this child considerable improvement has taken place during the last year, under treatment which will presently be decribed; for he can now run somewhat freely, and make *small* jumps from the ground in a manner which neither of the former ever could do. He is, moreover, able to walk up stairs slowly without the assistance of his hands.

In 1849 a fourth son was born, smaller and thinner than the other three, and altogether unlike them, so that it was hoped he might escape their fate; but the lower extremities are certainly becoming thick, and the child seems heavy, and a dead weight on the arms. Still Dr Meryon does not think that his present state would excite the notice of any person unacquainted with the history of his brothers. These are the only boys in the family.

There are six girls, aged respectively, 17, 15, 11, 10, 8, 5; but none of them have shewn the slightest tendency to the same form of disease. Two, however, have been the subjects of mesenteric disturbance; and this fact, combined with the usual physical characteristics of a strumous constitution in the three eldest boys, induces Dr Meryon to suppose that tubercular matter might have been deposited on the investing membranes of the spinal cord, or in the cord itself. In this idea Dr Meryon is supported by the researches of Lavéillé, Gendrin, Andral, and Ollivier, who have not only determined the fact of such tuberculous deposits in and around the nervous centres, but also that such disease is not common before the age of two years, after which, until puberty, it is of frequent occurrence.

A case reported to the Med.-Ch. Society, by Mr Stanley, and recorded in the Medical Gazette of February 7th 1840, shews that such gradual diminution and loss of muscular power may result from disease of the spinal cord independent of injury; another case which fell under the observation of Dr Budd, affords similar testimony; but in the first case related, the spinal cord and nerves connected with it were carefully examined, and not the slightest trace of disease was detected. The relative proportion of the gray matter to the white cord, the ganglionic cells of the former, and the tubular structure of the latter, as well as of the nerves and the white substance within the neurilema, wherever examined by the microscope, all bore evidence of the healthy condition of the nervous system.

The nervous tissues had preserved their integrity; and the only structural change observed was that which presented itself in the muscular fibres, which were broken down, and converted into granular and fatty matter.

Now, in ordinary paralysis, the fat which accumulates in the muscles merely insinuates itself between, and occupies the place of, the muscular fibres; but in the fatty degeneration here referred to, as in fatty disease of the heart, so admirably described by Dr Richard Quain, "a fatty matter, composed of granules and small oil globules, occupies and fills the sheath of what was formerly muscular fibre." Here then we have two modes in which fat accumulates in the muscles, as Dr Quain has described, in corresponding conditions of the heart. In the one case, the fat globules are deposited on the outside of the fibre, and in the other, the muscular fibre itself degenerates into granular and fatty matter.

This metamorphosed condition was so universal in the case in question, that it appeared in every voluntary muscle which was examined, except that the process of destruction had not advanced so far in the upper extremities and neck, where the transverse striæ were somewhat apparent, as in the abdomen and legs, where the striæ had almost entirely disappeared. Mr Quekett examined several portions of the muscles, and corroborated the observations made by Mr Tatum and Dr Meryon. In the muscular fibres of the heart, the transverse striæ were as discernible as in ordinary healthy hearts.

A case similar is the following.

2. Mrs H— had a very delicate constitution, and had been seriously ill just before Mr H. married her. She died eventually of peritonitis, the effect of tapping an ovarian cyst.

The eldest son, E. H—, was born whilst his mother was very ill, was partly nursed by her, and very badly nursed by others. He could not stand when he was two and a half years old, and at this time there appeared to be an entire cessation of growth; but about a year and a half after, he began to grow again slowly. He was, however, always *conspicuously* weak, as was also his brother; and in both it was observed that they never ran, jumped, or played with other children.

When between eight and nine years of age, they both manifested the same kind of muscular weakness which Dr Meryon has described in other cases, except that the arms were affected before the legs. In both the diminution of power was most marked in going up stairs, which was effected by laying hold of the balusters with both hands, then leaning the head on the hands and fixing themselves in that bent attitude, they dragged their legs after them. They were taken to Unterlacken, and placed under the care of Dr Gugenbühl, where they improved at first and grew; but the walking powers soon diminished again, and eventually both became perfectly helpless. The eldest died during an attack of measles, of pneumonia, and was examined by Mr Partridge and Dr Farish, when the lungs were found partially hepatized; all the other viscera were healthy, the nervous centres apparently so; but all the voluntary muscles were wasted, retaining a fibrous appearance, and were flabby and of a yellowish colour.

Dr Meryon was informed by Dr Farish that the muscles generally appeared to have degenerated into fatty matter; but they were not examined by the microscope. The case is reported in the "Medical Gazette" for November 1847, by Mr Partridge, who states that "the deltoid and sternomastoid muscles had undergone fatty degeneration. The calves (which were larger than natural, and had, during the progress of the paralysis, become permanently contracted), presented a greater degree of fatty degeneration in their muscular structure than the upper extremities, the soleus and gastrocnemius being more affected also than the flexor longus pollicis; neither the nerves nor tendons had undergone change."

The father informed Dr Meryon that just before the loss of power became apparent, the limbs grew thick and heavy; from him also Dr Meryon ascertained that he had two daughters, who were small in stature, but healthy and perfectly free from the disease in question. In these two cases, the commencement of the paralysis in the upper extremities renders the spinal origin improbable. It is, in the opinion of Dr Meryon, more likely that there was an original weakness, or deficient action of the assimilating organs, or, in other words, an absence of those elements and influences which are necessary for a healthy organization; antecedents which occurred to produce the fatty degeneration; or, as Dr Quain expresses it, "when the vital power which belongs to these higher products of animal organization is weakened or destroyed, they yield to the physical and chemical influences which surround them, and by an inherent principle descend into a class which is shared by them in common with plants and minerals."

3. Two more cases of a similar kind have fallen under the observation of Dr Meryon during the last year.

Two brothers, named Tyrrel, living in the vilage of Playden, in Sussex, began to lose the muscular power of their legs when they were about twelve years of age, from which epoch they gradually became worse, and when they were twenty their legs were useless; their arms then began to fail them, and at the age of twenty-five both legs aad arms were totally disabled. One of these was admitted into St Thomas's Hospital, under the care of Dr Barker, in 1847. There was a third brother similarly affected, who died some years ago, but, unfortunately, was not examined. A sister is living and perfectly well. As children, they were all ill fed.

Mr Quekett has observed certain facts which tend to shew that the malassimilation (if such be the cause) may be induced independent of original weakness. Mr Quekett informed Dr Meryon that the ostrich is peculiarly obnoxious to this fatty degeneration of the voluntary muscles, and that he has examined some which have died in the Zoological Gardens after long confinement. When once they drop, the keepers know that recovery is hopeless; and in all cases the muscular fibre is destroyed, and the sarcolemma broken down, as in the case related.

The only diagnostic signs by which this disease can be distinguished, are,—1*st*, its very gradual and almost imperceptible progress, differing from the paralysis agitans in the absence of tremulous agitation; 2*dly*, the entire absence of symptoms which denote nervous disturbance, as well as of paralysis of the bladder and sphincter, which usually accompanies paraplegia.

The following are the indications which Dr Meryon has kept constantly in view.

1*st*, To improve the quality of the blood, by giving it those elements in which it appears to be deficient, and diminishing those on which the deposit of the morbid matter depends. It may be, and most probably is, impossible to re-establish that nice adaptation of the blood to the formation of healthy tissues when once destroyed, for the very reason that the same principle of assimilation is at work in the reproduction of diseased tissues as is in operation in health in the reproduction and preservation of healthy tissues. But, as it is the character of a true philosopher to hope all things not impossible, and believe all things not unreasonable, so is it the character of the physician to hope and believe that the term *vis medicatrix naturæ* does not represent a nonentity; that there is in the animal body, when in a state of disease, a tendency to return to its healthy state, and that he has means and appliances to assist this curative process. Thus the condition of atrophy is known to be coincident with a deficiency of red corpuscles of the blood, and that iron has the property of restoring them. The poor, unhealthy blood, moreover, is deficient in plastic and organizable fibrin, hence a diet consisting chiefly of the fibrinous elements, would appear to be the most appropriate; whilst such habits of life as are conducive to perfect health are most likely to improve the function of nutrition, and so meet this first indication.

2dly, To ensure a regular supply of blood for the constant and regular regeneration of the muscular tissue. To this end nothing is more essential than an active capillary circulation in the interstices of the muscular fibres, and nothing more assists such function than well regulated muscular action; exercise of all the muscles is of paramount importance, and when the disease is far advanced, artificial exercise should be substituted.

3dly, To preserve a healthy influence of the nervous system, the regulator (as Volkmann has shewn) of muscular movements. Those electric currents which, according to Matteuci, are altogether independent of the nervous system, but produced by the molecular interchanges in the muscles themselves, exert a healthy influence on the nervous system by maintaining its function; and when disease has disabled the muscular fibre, nothing appears to excite it artificially so much as electricity, the interrupted current seeming to produce more beneficial results than the continued.

The 4th condition, which Mr Paget has enunciated as necessary to healthy nutrition, namely, " a natural state of the parts to be nourished," so entirely presupposes the fulfilment of the other three, that if they are accomplished it must necessarily be so too.

2. *On the Diminution of Chlorides in the Urine, or their absence from that Fluid in cases of Pneumonia, and on the Composition of the Sputa in that Disease.* By Lionel Smith Beale, M.B., London. Medico-Chirurgical Transactions of London, vol. xxxv., p. 325. London, 1853.

In August 1850, Dr Redtenbacher published some observations on the absence of chloride of sodium from the urine in cases of pneumonia. This physician noticed that the quantity of chloride gradually diminished until the period of hepatization had occurred, when no traces whatever of the presence of the salt could be detected in the urine, but the chloride again made its appearance as the resolution of the inflammation proceed.

A diminution in the quantity of chloride of sodium in the urine of various inflammatory diseases was remarked by Franz Simon; and all observers agree that in inflammations generally, the urine contains a greater relative amount of organic constituents than in health; while, on the other hand, the inorganic salts suffer a remarkable diminution.

This decrease in quantity of the inorganic salts is, doubtless, in part due to the altered diet taken by patients suffering from acute inflammatory diseases; but it will presently be shown that such an explanation alone will not suffice to account for the facts observed. In inflammatory attacks generally, although the chlorides may suffer diminution in quantity, or are altogether absent, this is not *constantly* the case, neither does the salt disappear at any particular period of the inflammation; but it has been shewn by the observations of Dr Redtenbacher in eighty cases of pneumonia, that in this disease the chlorides are invariably absent, and that the salt disappears from the urine at the precise period at which hepatization occurs in the lung.

Dr Beale has undertaken an extensive series of experiments on some cases of pneumonia in different stages, two of which, the fifth and ninth, terminated fatally, at once to ascertain the correctness of the opinion, that the chlorides are absent from or diminished in the urine, and to determine the relation between this fact, and the degree and stage of the disease.

The results are all that can be given here.

From the examination of the urine in the first case, terminating in recovery, the following facts were ascertained.

1. A considerable relative diminution of the quantity of the salts generally, until the patient progressed towards convalescence, when the amount again increased, until, on the twenty-second day, the quantity was found to exceed that present in health.

2. A total absence of chloride of sodium from the urine (with the exception of one day, on which traces were found), until convalescence was established.

3. The presence of albumen in the urine from the fourth day of the disease to the sixth inclusive (during which period the pneumonia was at its height). On the tenth day albumen could not be detected in the urine.

In the third case of pneumonia, which terminated in recovery, the following facts were observed.

1. A diminution of the fixed saline matters, generally, in the urine, and and an increase of the organic constituents.

2. A total absence of chloride of sodium in the urine from the fifth day of the disease to the tenth day, when the patient was convalescent, and symptoms of hepatization were no longer to be detected.

3. A remarkable increase, compared with the quantity present in a state of health, in the amount of fixed saline matter in the sputa of the ninth day (the rusty character having disappeared on the previous day), and the presence of a considerable proportion of chloride of sodium.

The determination of chloride of sodium to the inflamed lung in cases of pneumonia, is perhaps a fact, which, from analogy, we should expect to find; for, from the analyses of various observers, it appears that a large quantity of chloride of sodium is present whenever the metamorphoses of tissues depending upon cell development is going on; and that such increase of the chloride should be observed to take place in those pathological conditions in which cell formation takes so active a part, is exactly what one would expect, seeing that the farther investigation is carried, the greater reason do we find for believing that the pathological changes which are observed in various tissues are governed by, and take place in obedience to the same fixed laws which regulate certain physiological changes, but acting under different relations.

In reference to this interesting point it may be remarked generally, that in a state of health those secretions which are elaborated by an abundant formation of epithelium contain a greater proportion of chloride of sodium than is present in those fluids which are poured forth from surfaces covered sparingly with epithelium, which, perhaps, takes but a small part in the secreting process. Of instances of the former class of secretions, may be mentioned the saliva, the gastric juice, the sweat, the tears, and mucous secretions generally,—among the latter class, the milk, and the secretions of serous and synovial membranes in health, may be referred to as examples. Urine may be placed intermediately between these two classes, and biliary secretion is found to contain less chloride than urine; but much more than is found in milk.

In the development of tissues in the embryo, a large proportion of fixed chloride is present. In the growing femur of a six months' fœtus, Lehmann found 10·138 per cent. of chloride in the ash, while from the ash of adult bones he could only extract from 0·7 to 1·5 per cent.; and Voigt found in the liquor amnii of a fœtus, about the fourth month, 5·95 per 1000 parts, while in the sixth month he only obtained 2·4 in the same quantity.

The peculiar sallowness of the complexion which has often been observed to ensue after the occurrence of hepatization of the lung, in cases of pneumonia, may be due, in part at least, to the altered colour which the blood assumes in consequence of a deficient quantity of chloride of sodium, for the mere circulation of blood which had been but imperfectly aerated, would not account for the chlorotic appearance which is often present. A deficiency of choloride of sodium in the blood has been observed by Mulder, and others, in cases of cholera; and a marked diminution has been met with in chlorosis, by Becquerel and Rodier, who found only 2·6 grs. per 1000 in one instance of this disease, in which, however, the corpuscles amounted to 123·8, and the solid matter to 201·4 per 1000.

The most important points which Dr Beale has endeavoured to establish are the following.

1. That in pneumonia there is a total absence of chloride of sodium from the urine, at or about the period of hepatization of the lung.

2. That soon after resolution of the inflammation, the chloride becomes restored to the urine, and often in considerable quantity.

3. That at this period the serum of the blood is found to contain a greater amount of chloride than in health.

4. That the presence of chloride of sodium in the urine may be taken as evidence of the existence of a greater quantity of the salt in the blood, than is required for the wants of the system generally, or at least of an amount sufficient for that purpose; and that the absence of the salt from the urine indicates that the circulating fluid contains less than the normal quantity.

5. That the sputa in pneumonia contain a greater quantity of fixed chloride than healthy pulmonary mucus, if there be not much less than a normal amount in the blood, although there be a complete absence of the salt from the urine. In all cases, however, there is found in the sputa a quantity many times greater than exists in an equal amount of blood at the same period of the disease. The absolute amount present is subject to variation at different periods of the disease, and in different cases.

6. That in case ninth, as the disease approached a fatal termination, the proportion of chloride present in the sputa underwent a decrease, while the amount of solid matter, and especially the extractive matters, increased in quantity. At the same time the sputa became acid, and in the matters expectorated within the last few hours of the patient's life, there existed evidence of the presence of a large quantity of grape sugar, but in that obtained on the day previous to his death none could be detected.

7. That there is reason to believe, that the absence of the chloride of sodium from the urine during the stage of hepatization, depends upon a determination of this salt to the inflamed lung, and that when resolution occurs, this force of attraction ceases, and whatever salt has been retained in the lung is reabsorbed, and appears in the urine in the usual way.

3. *Account of a Case of Biliary Fistula.* By George Robinson, M.D., Newcastle-upon-Tyne. Medico-Chirurgical Transactions, vol. xxxv., p. 471. London, 1852.

Elizabeth Balmbra, aged 64, the wife of a pitman, an active and generally healthy woman, the mother of seven children, had ceased to menstruate nineteen years since; and with the exception of occasional pains in the legs and lower part of the back, she continued to enjoy good health till within the last three years. She was then, for the first time, seized with pain in both sides extending to the epigastrium, of a paroxysmal character, each attack lasting about twenty minutes, and being preceded by rigors. During the intermission of the paroxysms, there was always more or less pain in the hypochondriac and epigastric regions, increased on pressure. For the relief of these symptoms, the surgeon in the colliery village where she then resided give her bitters, taraxacum, and cod-liver oil, and applied blisters to the seat of pain; opium in full doses being both then and subsequently also administered during the paroxysms.

In the month of October 1852, she came to reside at Dunstan, near Gateshead, and became a patient of Mr H. R. Stevenson, by whom the attention of Dr Robinson was afterwards directed to the case. She was then in a state of intense jaundice, the motions being white, the urine and skin deeply stained with bile, and the humours of the eye probably similarly tinged, as all objects appeared to her of a yellow colour. The rigors and subsequent paroxysms of pain were very frequent, and there was a hard tumour, of the size of a man's fist, in the epigastric region, two or three inches above the umbilicus, with obscure swelling in both hypochondria. At no period of the disease was there any tendency to coma or delirium, nor was vomiting ever present, though at one time frequently solicited by copious draughts of warm water. The tumour gradually softened; exhibited fluctuation and pointing; and, by the commencement of January 1852, its cavity had so nearly approached the surface, the skin covering it being very thin and inflamed, that it was deemed advisable to relieve her sufferings by opening the abscess, which was done with a common lancet, and at least two quarts of a bright yellow, very offensive liquid, evidently a mixture of bile and pus, were discharged. The patient was at once relieved by the evacuation of the abscess; no constitutional disturbance followed, and she was placed on good diet, with beer, and cinchona. Under this treatment the discharge, which was at first very profuse and of an offensive smell, gradually diminished in quantity, and became clearer, thinner, of a green colour, and void of any offensive smell, in fact resembling in its physical properties pure bile; while the opening in the epigastrium made by the lancet also simultaneously contracted.

On the 18th of March 1852, when carefully examined by Dr Robinson, she had so far recovered as to be able to walk about the house and attend to light domestic duties. Her chief complaint was of the discomfort occasioned by the constant flow of bile through the opening in the epigastrium, which completely saturated her linen and the cloths applied, and stained them of a deep saffron colour. The fistulous aperture was situated in the median line, two inches above the umbilicus, and was of the size of an ordinary goose-quill. A small stream of bile was constantly flowing through it, and when collected was very pure, but occasionally contained a very few flakes of pus. By means of a suitable glass retained over the opening, a considerable quantity of bile was collected, and an estimate formed of the average amount daily discharged, which was fixed at about eight fluid ounces. In order to place the nature of the fluid beyond doubt, a quantity was forwarded, collected on two successive days, to Dr Bence Jones, who pronouced it to be pure bile. The rate of discharge from the opening varied on different days, and the bile flowed most copiously when she was in an erect position; the flow was also more rapid after her meals, and was increased by straining or coughing. Her general health was at this time very good; her pulse 90; her appetite, which at first had been voracious, was now more moderate. With the exception of flatulence, she experienced no symptoms of indigestion: to use her own words, "She did not feel or know she had a stomach." Like other persons in the same station, she generally lived upon milk and bread, with tea, fish, and occasionally meat. She had partaken of chicken and beef-steak without any inconvenience. The bowels were regular, acting once or twice daily. Within a week after the abscess was opened the jaundice had disappeared as regards the colour of the skin and urine, and surrounding objects no longer appeared to her of a yellowish colour, nor have these symptoms since returned. The intestinal evacuations, however, have never resumed the natural appearance, being still very light-coloured and evidently destitute of bile. The right hypochondriac region was free from pain, but on pressure over the left side there was still a little tenderness, and round the fistulous orifice there was a distinct induration to the extent of an inch and a half. On drawing a very full inspiration, there was also a sense of pain about the epigastrium, as if from the stretching of the adherent parts.

After seeing this case a few times, Dr Robinson suddenly lost sight of the patient, about the end of March, in consequence of the male members of her family having found employment in another colliery, to which they had migrated; and, on ultimately discovering her new residence, on the 25th of May, Dr Robinson found her very much reduced in strength, but still presenting all the phenomena previously noticed in connection with the biliary fistula. The appearance of the orifice and the absolute quantity of bile discharged are the same, though from her constant recumbent posture, the rate of discharge in a given time is more variable than when she was erect. She is at this time, June 4th, very much emaciated, which is partly due to the withdrawal of the beer, and the absence of adequate nourishment, though the biliary discharge doubtless adds to the debility.

A case somewhat analogous to this is mentioned by Dr Heberden, in his Commentaries.

"A woman, fifty years of age, was for ten days severely afflicted with pain of the stomach, hiccup, purging, and faintings, and with difficulty struggled through it. A month after there arose a swelling near the navel, which was opened, and discharged a great quantity of yellow fluid for the space of four years; at length the pain increased, together with sickness and shivering; and, after a few days there was discharged a gall stone, three inches long and as much in circumference, weighing 245 grains. During the two following weeks a thin liquor was poured out in great abundance: soon after the sore healed up and the woman recovered." (4th ed., p. 210.)

This patient died on July 12, 1852, and with some difficulty permission was obtained to examine the chief seat of disease. The liver was firmly adherent to all the surrounding structures, and its upper and anterior surface was attached by very strong adhesions to the abdominal parietes, at a point corresponding with the external fistulous orifice. The latter was found to communicate with one of the minor biliary ducts, all of which, however, were greatly dilated, as was the ductus hepaticus, which readily admitted the fore-finger. In the ductus communis choledochus, immediately below the junction of the hepatic and cystic ducts, was imbedded a calculus of cylindrical form, convex at one end, and regularly concave at the other, the former being directed towards the hepatic, the latter towards the intestinal, extremity of the duct. This calculus, three-fourths of an inch in length, and two-thirds of an inch in its transverse diameter, was of a saffron colour, exhaled the peculiar smell of bile, and was evidently composed of that secretion in an inspissated state. The coats of the duct were tightly stretched upon it, and the obstruction to the flow of bile from the liver to the duodenum must have been complete. As the calculus was situated at a considerable distance from the fistulous opening in the epigastrium, a portion of the liver intervening, it is evident that any surgical exploration or enlargement of the orifice, which was at one time thought of, would have been worse than useless.

Dr Bence Jones found that the calculus consisted of inspissated bile, and he thinks there must have been another in the gall-bladder. 10c.

The foregoing case is probably to be referred to the same head, pathologically speaking, as that of instances of gall-stones escaping through the abdominal parietes by ulceration. In both instances the fistula is formed; but in the case above detailed the gall-stone remains in the passage, its progress to the surface being interrupted, whereas in the cases referred to it completes its escape. Of this mode of termination of escape of gall-stones many instances are recorded.

II.—STATISTICS.

PARIS HOSPITALS.

The following statistical details are extracted from the recent report made to the Administration of Public Assistance for 1851 :—The hospitals of Paris are divided into two categories, general and special.

The general hospitals are nine in number, containing together 3706 beds—viz., Hotel.Dieu, 810; St Marguerite, 355; La Pitié, 624; La Charité, 494; St Antoine, 290; Necker, 252; Cochin, 125; Beaujon, 438; and Bon Secours, 318. These nine establishments are without distinction appropriated for the treatment of wounds and for an innumerable variety of acute diseases.

The special hospitals are exclusively reserved for the treatment of affections of a particular kind. They are six in number, and contain together 2723 beds; viz., St Louis, 853; Du Midi, 336; Lourcine, 600; Enfans, Malades, 600; Maison d'Accouchement, 514; and the Hôpital des Clini ques, 120.

To these must be added the Maison Nationale de Sante, where the entry is not gratuitous. It was founded in 1802, and its object is at a certain price per diem, to give either a private room, or a place in the common sleeping-room, to patients who cannot receive proper treatment at their own homes, but who, having some resources, do not wish to have recourse to public charity. All diseases which are not contagious, incurable, or mental, are attended to there, and, in its present state, the establishment can receive 150 patients of either sex. During the year 1851, the number in the general hospitals was 41,192; in the special hospitals, 16,377; and in the Maison de Sante, 910; making together 58,487 patients.

The surgical cases in the three establishments were respectively 14,636, 8421, and 779, making a total of 82,323. Taking into account those who were in the establishments on the 1st January 1851, the number of surgical cases during the year amounted to 86,775. Of these 73,948 left the houses cured or convalescent; 7186 died, and 5641 remained there on the 31st December.

The average mortality among the medical cases in the general hospitals varied from 1 in 6·94 to 1 in 11.58, and in the different establishments taken together, 1 in 8·66. In the special hospitals the mortality among the medical cases varied in a greater proportion; it was 1 in 5·40 in the Enfans Malades, 1 in 43·23 at St Louis, and 1 in 4·65 in the Maison de Sante.

OBITUARY.

On Wednesday the 6th of April 1853, Richard Chambers, M.D., died at his residence, Wimpole Street, London. As death was sudden and unexpected, though Dr Chambers had previously been not quite well, an inquest was directed to be held.

On Monday 11th April 1853, the Coroner held an inquest on the body. Mr Wilson of Charlotte Street, who made the examination of the body, said, that on opening the body he was struck, as also were the other gentlemen present, with the odour of prussic acid. He found the right ventricle of the heart double its natural size. The phial found beside the deceased smelt strong of prussic acid; and the prescription according to which its contents had been compounded, the deceased having written the prescription, contained six drops prussic acid, Scheele's strength, ten drops Battley's solution of opium, one drachm of colchicum, two of acetate of ammonia, and one ounce and a half of water. Mr Wilson expressed his belief that the deceased had taken this as a medicine, and that owing to the disease of the heart, the prussic acid had caused death. The jury, after hearing all the evidence, found that the death of the deceased was caused by diseased heart, under the influence probably of prussic acid taken medicinally.

It seems very doubtful whether the Hydrocyanic Acid in this case was the cause of the fatal termination. It is more likely, so far as a judgment can be formed, that death took place in consequence of the diseased state of the heart and the lungs. For that the lungs were in a morbid state must be inferred from the condition in which the right ventricle of the heart was found. Such a state of the right ventricle could not exist without great obstruction to the circulation of the blood through the lungs.

On the other hand, it may be argued that the administration of Hydrocyanic acid, in such a state of the heart and lungs especially, was a questionable, if not an unsafe procedure. If there were an impeded state of the pulmonary circulation, and if the heart was already taxed to its full power, the temporary but instant paralysis induced by Hydrocyanic acid, might be sufficient to annihilate any little remaining power.

At the same time it must be acknowledged that the cause of death is obscure. No farther information was given, except that Dr R. Chambers was found dead in bed.

Shortly after the publication of last Number, the Profession in Ireland sustained a great loss in the death of Robert Graves, M.D., an event which took on the 20th April, at his house in Dublin.

Dr Graves was a person of great talent and no moderate degree of activity in his profession. Above twenty years ago, 1828–1830, he conducted, in conjunction with Dr John Cheyne, the publication of the Dublin Hospital Reports, and contributed to that work several useful papers.

In March 1832 Dr Graves began the publication of the Dublin Journal of the Medical and Chemical Sciences, and conducted that work for several years with great zeal and industry. Indeed, by his example and his influence with his friends, especially Dr Stokes, he may be regarded as the animating spirit of that valuable periodical.

In 1843, Dr Graves published his Lectures on Clinical Medicine, a work containing a large amount of information on the Pathology and Treatment of Diseases. A second edition of this work was published in 1848 by Dr Moore Neligan. Dr Graves continued to communicate various papers to the Quarterly Dublin Journal to the last; and of one of the most recent of these, we think the most recent, an abstract was given in the last volume of this Journal.

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PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*Upon the Influence of the Nervous System on the Movements of the Iris.* By Professor Julius Budge of Bonn. From the German. [Aus dem Archiv fur Physiologische Heilkunde; Ergänzungsheft, 1852.] From the Archives of Physiological Medicine. Supplementary Number, 1852.

[Concluded from No. 196, page 23.]

Above the uppermost cervical ganglion the experimenter can, along the whole further course of the Sympathetic Nerve, which enters into the ciliary ganglion, induce dilatation of the pupil by irritation of this ganglion ; that is, consequently, when he traces the internal carotic branch into the internal carotic plexus ; because when he follows it to the substance of the Gasserian ganglion, he also touches the ophthalmic branch with the poles. In this manner it is only possible to experiment after death, because the exposition of the fibres specified is necessarily followed by death.

The irritability of this upper portion of the Sympathetic Nerve remains a considerable time after death, so that the operator is usually able to expose the united nerves with all deliberation. The branches which proceed to the Gasserian ganglion, lie, it is well known, in the substance of this ganglion. We hence witnessed more frequently first the action, when we made the wire penetrate into the interior of this ganglion; while touching the surface was followed by no effect. It is thus very interesting to observe how an irritation which is produced close to the nerve, yet does not touch it, no longer causes dilatation some time after death. But as soon as the operator places the nerve in the chain he forthwith beholds the phenomenon.

By means of our experiments consequently was indicated the whole path which is passed by the nerve enlarging the pupil, from the spot where it lies deep in the neck with the windpipe, to the ciliary ganglion.

The next question for resolution must be to trace it further, until we are able finally to indicate the limiting point, beyond which any action of the pupil is no longer to be observed.

For this purpose, to the chord of the sympathetic nerve irritation was applied immediately after death, first in the abdomen, and then in the chest, in different animals. But never could we observe the smallest influence through this medium upon the action of the iris. Of this fact we had also convinced ourselves in two animals still in life, which had previously been deprived of sensibility by means of ether; namely, in a rabbit and in a dog. In both we succeeded in laying bare, not only the lower cervical ganglion, but also a portion of the sympathetic nerve beyond this ganglion, and in this manner we applied irritation. In the case of the dog we cut through the nerve; but in the rabbit this was not done. The experiment was more difficult and more troublesome to perform in the rabbit, because usually the ganglion lies at a greater depth, and the chord is thinner than in the dog, in which the two nerves lie united with each other. After we had succeeded in one rabbit, in placing upon one glass rod the lower half, that is the thoracic portion, and upon a second glass rod the part of the chord of the sympathetic lying immediately above, to one and both spots the wires were repeatedly applied; and the interesting fact was here shewn, that dilatation of the pupil only ensued, when the portion lying above the ganglion was exposed to irritation, but not when the operator acted in like manner with that below the ganglion. The ganglion, in short, formed the definite boundary. In this manner, therefore, the limits were ascertained, and the route which the sympathetic nerve takes in dilating the pupil, became accurately known.

At this point, however, of the inquiry it was impossible to remain stationary; because the question is suggested, Where arises this part of the sympathetic nerve? Three conditions may be conjectured to take place. First, it might be conjectured that the sympathetic nerve in the neck may be the continuation of the sympathetic nerve in the chest, but that the irritation applied to the latter may produce no action upon the pupil, though even between them lie the ganglions, which have been considered as organs that interrupt the conduction of nervous energy. Physiologists have not unfrequently considered the ganglions as isolators of the nervous current, and consequently the same opinion might be entertained in the case of our experiments. On the other hand, they have, in the whole body, adduced not one fact to shew that a complete cessation of nervous conduction takes place from removal of an intervening ganglion, or even that such a result is probable. But the circumstance which principally deserves consideration is this, that in the course of the sympathetic nerve, there are yet three other ganglions, namely, the upper cervical ganglion, that of the Fifth pair or Trigeminal nerve, and the Ciliary ganglion, by means of which the same conduction ought to be protracted and removed farther off; that these ganglions impair the force of the action. We have, according to the experiments which are above mentioned, and which are yet to be narrated, rather reason to conjecture the contrary. This first hypothesis consequently must be set aside, as in the highest degree improbable.

There yet remain two other possible propositions, namely, that this part of the Sympathetic nerve arises from the inferior Cervical ganglion, or the third from the Spinal Chord. If the sympathetic nerve derives its origin from the ganglion, the operation of cutting through the nerve below the ganglion ought not to induce contraction of the pupil. Because the contraction of the pupil arises from this circumstance, that the sympathetic nerve is separated from its centre. See § IV. But if the ganglion is the centre, then in the condition supposed, no contraction ought to follow.

We deprived of sensibility by means of chloroform a dog in one experiment, and then searched for the lower cervical ganglion and cut through the trunk below the ganglion, as also all the filaments connected with the ganglion. Of all the filaments only one acted upon the pupil; from the others we observed no result. We formed the conjecture that this filament is a communicating branch between the nerves of the spinal chord and the inferior cervical ganglion. Soon after the section of the trunk we saw contraction of the pupil take place, exactly as when the sympathetic nerve has been divided in the neck.

Hence the probability was increased, that in the spinal chord itself there might be found a spot, by the irritation of which the same result might be expected to take place as arises from the irritation of the sympathetic nerve in the neck.

Previous observations which I had made, shewed, that by irritation of the spinal marrow, the action of the stomach and bowels may be induced. Subsequent experiments which I made in the year 1850, taught me that from the lumbar region of the spinal marrow we can withdraw from action the sexual organs in rabbits. I was very desirous to be able to make researches upon this point in an organ which can be observed without further injury, like the Iris.[1]

In a rabbit which had been previously subjected to the full narcotizing influence of ether, and in which the sympathetic nerve was divided on the left side, we opened on the 4th of August 1851, the vertebral canal in the region of the third dorsal vertebra up to the cervical vertebræ. After we had cut across the spinal marrow below the dosral vertebra, we applied galvanic irritation to the dorsal spinal chord above the point of section. Immediately the pupil on the right side was dilated, while that of the left side on which the sympathetic nerve had been cut through, was not changed in the smallest degree.

After we had obtained this highly striking result, it occurred to us thereupon to determine the limits within which the application of an irritating agent still acts upon the eye. Upon this point we had performed a great number of experiments upon rabbits, and had seen that these limits are not similar in every animal. But in none of our experiments could we at any time observe a change in the pupil when the pole touched the part of the spinal marrow which lay below the sixth dorsal vertebra, that is towards the loins. We have, for example, laid bare in one rabbit the whole lumbar portion of spinal marrow, and applied irritation to spot after spot; yet the pupil has in such circumstances retained its previous diameter. We in like manner laid bare the posterior part of the dorsal portion of the spinal marrow from the thirteenth to the sixth dorsal vertebra; and here also we saw no action. As soon, however, as we had passed the sixth dorsal vertebra, dilatation of the pupil took place when the irritation was applied. We proceeded thence further forward, and could still uniformly observe effects as far as the fifth cervical vertebra, consequently for a length of about eight vertebræ.

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[1] These experiments were witnessed in a male rabbit by Messrs Wutzer, Froschel, Ungar, and many young physicians, in November 1850.

These experiments were witnessed at the same time by many scientific associates, among whom were not only many physicians of this place (Bonn), the most of the professors of the Medical Faculty of Bonn, but also eminent strangers, as Scanzoni from Würzburg, Rölle from Vienna, Fisher from Cambridge, Nasse from Marburg.

The experiment never fails. It is, however, not possible to observe the action in every animal, either as far forward or so far back as the extreme limit anterior and posterior is represented above. There is, on the contrary, one spot at which the operator is most certain of obtaining the result, from which he can at length effect the action, in all probability the centre for this portion of the spinal marrow, which exerts such influence upon the pupil. This part corresponds to the first dorsal vertebra. The nearer the operator approaches to this spot, the more distinct and the more vigorous is the dilatation of the pupil, in consequence of the application of an irritating object. In one rabbit, with the view of obtaining more accurate information, we took out the spinal chord from the fifth cervical vertebra to the sixth dorsal vertebra, and inserted the wires in the intervertebral Foramina. There was found only one single intervertebral foramen, from which the application of irritation produced effect, and this lay between the second and the third dorsal vertebræ. In the others no result was witnessed.

This spot, nevertheless, according to experiments which I subsequently performed, it is impossible to fix in a decided manner, and to maintain that it is only the second dorsal nerve, from which the Sympathetic Nerve proceeds; but I have already seen with certainty that the first dorsal nerve as well as the second is in this condition; and probably there are some more. So far only, I believe, can we positively assert, that the central portion for the Sympathetic Nerve is placed in that part of the Spinal Marrow, which lies between the last cervical and the first three dorsal vertebræ.

I here give a short description of the connection which subsists between the Spinal Nerves now referred to, and the Sympathetic Nerve in rabbits.

The anterior branch of the second dorsal nerve gives off, as soon as it comes in sight upon the posterior costal surface, two small connecting branches to the Sympathetic Nerve, which go over to the second thoracic ganglion. It then sends a branch upwards to the anterior branches of the first thoracic nerve; and from these communicating branches between the two thoracic nerves there ascends then inwards a connecting twig or branchlet, runs a short space along with the

Sympathetic nerve, and passes into the Plexus; from which fibres proceed to the first thoracic and to the last cervical ganglion.

From the anterior branch of the first thoracic or dorsal nerve, fibres proceed something downwards, and are united with the deep parts of the first thoracic ganglion; others, again, proceed upwards, to be united, partly with the first thoracic ganglion, partly with the last cervical ganglion, which lies very close to them.

Besides this, the last cervical ganglion receives branches also from the eighth cervical nerve, and even from the seventh and the sixth cervical nerves.[1]

According to these observations, the question next presents itself, In what manner is the spot of the spinal chord, through irritation of which the pupil is dilated, associated with the Sympathetic Nerve in the neck? Are there fibres, that is fibres from the spinal marrow, which must be in some relation to the Sympathetic Nerve. This is the necessary consequence which flows from these observations. It must, however, become a matter of inquiry, at what point, if possible, by examining experimentally the whole course of the fibres, to ascertain and to obtain an explanation upon this point,—namely, whether the primary fibres of the Sympathetic Nerve in the neck are in uninterrupted connection with those fibres in the spinal marrow, or whether the irritation of the latter is reflected upon the Sympathetic Nerve. Any one who clearly comprehends these questions in all their bearings, will perceive their great importance, which the solution of the same exercises, not only in explaining the action of the Iris, but also in elucidating the origin of the Sympathetic Nerve. It behoves us therefore to subject to the proof of experiment all possible propositions, or at least those which correspond to the point as a question in science.

I have said, that we may conjecture that the action which we induce from the spinal marrow upon the Pupil, is of the nature of a reflex action. This opinion I form in the following manner. The ganglia which are connected with sensiferous roots might possibly act as central organs and become the intermediate parts between the fibres of the spinal marrow, the irritation of which, and the Sympathetic Nerve, causes dilatation of the pupil. In this case we must suppose that the fibres of the communicating branches of the Sympathetic Nerve, arise from the spinal ganglions, and not from the spinal marrow.

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[1] The detailed anatomical description of the cervical part of the Sympathetic Nerve and the Iris in the rabbit is to appear in the Transactions of the Natural History Union of Prussian Rhineland and Westphalia.

The fibres in the spinal marrow itself, which here are considered, and the fact of the posterior root from its origin to the ganglion, stand in this instance to the pertaining communicating branches of the Sympathetic Nerve, which constitute the dilating nerves of the Iris, in exactly the same relation, as the posterior roots of the spinal marrow do to the anterior roots thereof. The excitation of the first named sensiferous nervous fibres induces an excitation in the motific part of the Sympathetic Nerve, and the communication is effected through the medium of the spinal ganglia. Though this view does not correspond with the opinions formerly entertained by me, and I rather had the idea that the Sympathetic Nerve arises from the spinal marrow, yet in research our sole duty is to discover truth, and not to defend our own views. According to many recent microscopical researches, which have been made by Volkmann, Kölliker, myself, Beck, Ludwig, Wagner, Stannius, Bidder, and other observers, it is a point, though not placed beyond all doubt, yet very probable, that the peripheral gangliar primary Nerve-Fibres arise from the gangliar globules. These observations may lead to the proposition, that the peripheral ganglia are disposed in like manner to the central ganglia, though this does not necessarily follow therefrom.

In order to obtain a decided result upon this point, I have laid bare in rabbits the region of the spinal marrow, from which the superior thoracic nerves arise, and I have applied irritation to the posterior and anterior roots individually; and I have been fortunate in arriving at a certain result in this way, after many experiments had proved unavailing.

For the sake of those inquirers who propose to repeat these experiments, I feel it requisite to advert to a difficulty which presents itself in the performance of these experiments. This difficulty consists chiefly in this circumstance, that after the preparation of the parts, the irritability often vanishes with extreme rapidity, and then, that it does not cease simultaneously in different parts, though these may be situate close to each other, in consequence of which the operator has constantly to perform Correcting or Normal experiments, before he can venture to deduce conclusions.

I have found it most convenient to divide the Spinal Marrow above its dorsal or thoracic portion. After the animal has been deprived of sensibility by a mixture of ether and a small proportion of chloroform, and a longitudinal division of the skin has been made, I search for the fourth cervical vertebra, make in this region a transverse incision through the whole of the muscles, press in the same manner to the space between the fourth and fifth cervical vertebræ. The operator may at this point easily expose, without injury of the vertebræ, the spinal marrow, and then most easily, by a pair of strong scissors, cut it through. When this has been done, it is most advantageous not to carry the use of the ether any further, because by the division of the *Medulla oblongata,* the posterior part of the body is rendered quite insensible. The vertebræ are then exposed and their spinal plates broken off, so that the operator may be able to see the roots of the first two thoracic nerves. In order to avoid error as to the exact spot, the first rib serves as a guide; and this part may be denuded as far as it is easily felt.

It is deserving of remark, in reference to the operation of dividing the spinal marrow, that when the spinal marrow is divided so high up, normally after the section the posterior extremities remain perfectly tranquil. When, on the contrary the spinal marrow is cut through somewhere below the middle of the vertebræ, usually actions of the extremities take place immediately after the operation.

I placed a glass rod beneath roots of the two thoracic nerves; and upon these roots a practised assistant held the two closely-approximated wires. There were produced forthwith violent convulsions, by reflex action, in the posterior part of the body, and the pupil was distinctly dilated. The experiment was twice repeated with the same results; then the roots were divided close to the spinal marrow, and were spread upon a small piece of glass. A newly-applied irritation produced not the slightest result either upon the pupil or upon the rest of the body. The wires were then applied close to the motiferous roots of the same nerves on the same side; and instantly the pupil was very largely dilated, and on repetition of the experiment several times the same result took place. At length the action ceased, probably in consequence ef exhaustion.

I performed exactly the same experiment also with the two roots of the first cervical nerve, and I obtained exactly the same result. By applying irritation to the posterior and anterior roots the pupil is dilated. On the other hand, the experiment fails when irritation is applied, after the posterior root is detached from the spinal marrow.

Afterwards I separated this portion of spinal chord from the deep-seated parts, from which the second thoracic pair of nerves is derived; so that accordingly this small portion from which the first pair of thoracic nerves arises, was perfectly isolated from the spinal marrow lying before and behind it. In this isolated position the posterior root was cut through on the right side; and still, after irritation of these parts, dilatation of both pupils was evinced in the same distinct manner. As has been already remarked, the next following portion of spinal chord, with the second pair of thoracic nerves, as it had already been often subjected to irritation, had completely lost its irritability, and we could no longer in that induce any irritation. This circumstance particularly I adduce, that the action which is produced from the anterior thoracic portion, may not sometime be propagated from a continuation or extension of the galvanic fluid, in the first place, because both portions were perfectly isolated; and, secondly, because no result appeared to follow, after irritation of the posterior root, when separated from the spinal marrow.

No doubt, accordingly, can any longer be entertained, that the Sympathetic Nerve, which sends filaments to the Iris, really arises from the Spinal Marrow, and comports itself in like manner as the other Cerebro-spinal Nerves. Because, since the irritation of the motiferous roots causes dilatation of the pupil, while the sensiferous roots are no longer in connection with the Spinal Marrow, no other interpretation of the phenomena remains to be given, than that the irritation affected the fibres themselves which help to compose the Sympathetic Nerve. The experiments on frogs leading to the same results may be seen at § IV.

Agreeably to my experiments, I must now conclude that the dilatation of the pupil, which succeeds the irritation of the posterior roots of the first thoracic nerve, is nothing but a reflex action, which, like the other usual reflex actions are communicated through the Spinal Marrow, and not in any degree through the Spinal Ganglions. Because if the Spinal Ganglion might produce this effect, or if with the sensiferous roots were mingled motiferous roots, which supplied the Iris, irritation of the root separated from the Spinal Marrow, ought to have caused dilatation of the pupil, which did not take place in any of the experiments which I performed. If, however, I still recommend to consider this question as not yet completely determined, this is because, as I believe, the experimenter in experiments on the Spinal Marrow and the nervous roots, which are performed on Mammiferous animals, cannot be sufficiently careful how far these experiments furnish a negative result; and, in truth, for this reason,—that the irritability occasionally disappears in a remarkably speedy manner. For instance, I have in one experiment first applied irritation to the posterior root of the first thoracic nerves, and thereby have seen the action upon the Iris; then I have divided this root, and I have observed no result from the irritation applied to the anterior roots immediately afterwards. Forthwith I have subjected to examination the other half of the Spinal Marrow, which had not been injured; and here also every sort of irritation was ineffectual.

It is consequently possible, that, in consequence of the division of the posterior roots, the irritability in them is speedily extinguished. It is still to be observed, that, after the division, the roots are not elongated, but are rather contracted, and then management consequently becomes more difficult. I hence recommend, in repeating the experiment, to direct full attention to this point. It remains meanwhile in a high degree probable, that the action upon the pupil from the posterior roots is only a reflex action. The dilatation of the pupil, which occasionally accompanies affections of the abdomen, as worms, appears also to depend upon reflexion. It is known that conditions of the sensiferous nerves pass with extraordinary facility from the affected spots to other sensible organs. Thus it is well known that pain circumscribes not only the limits of morbidly affected parts of the body, and that neighbouring parts which are sound feel pain consentaneously, but pain arises even in corresponding nerves on the other side of the body. It may therefore be conceivable that the condition of sensation in the abdomen might involve the sensiferous nerves in community of suffering, which again may react upon the nerves which move the Iris.

It may also happen that, through irritation of parts of the Spinal Marrow, from which no fibres directly united with the Sympathetic Nerve proceed, nevertheless the pupil is dilated, as in point of fact is the case. If also it cannot yet be positively maintained, that filaments proceed only from the first and second thoracic nerves to the Sympathetic of the Iris, it is nevertheless not to be assumed that from the third, fourth, fifth, and sixth thoracic nerves, filaments go to those of the Great Sympathetic Nerve, otherwise irritation of this nerve in the chest would certainly act on the pupil; but this is not the fact.

May the action of the Iris, which depends on the Spinal Marrow, proceed only in the way of reflex action,—that is, through the medium of the posterior roots,—or may it proceed at the same time also directly from the motiferous fibres? Must there be always an impulse, which excites either the respective sensiferous or the motiferous nerves in both, wherewith the state of quiescence is overcome? What, again, is this impulse or stimulus? It has been impossible for me to discover any such direct irritating cause as light is in relation to the Optic Nerve and the Oculo-muscular Nerve. On the contrary, the proposition remains here admissible as for all corporeal nerves. Indeed it would be necessary, that all nerves are in a state of permanent energy, which, however, apparently does not take place, because antagonistic nerves hold the equilibrium. I think that it is not an inadmissible proposition that this permanent energy has for its formation a permanently operating cause; that is to say, that in the organization there exists an irritating or stimulating agent for the nerves, and that such must be in constant preparation. According to my conjecture, founded upon observations, it is the bile and the salts of the blood which represent these stimuli and irritating agents. But, whatever it may be, it cannot be wanting; because wherever we behold a distinct nervous action, we learn also to recognize some motific or exciting agent. This irritation which constitutes antagonism, if it is everywhere present, must act also upon the sympathetic fibres of the Spinal Marrow. The energy of these fibres can, however, so much less give rise to phenomena, the more irritation is opposed to the nerve which acts in an opposite manner, especially and chiefly the Oculo-muscular nerve. Of this last-named nerve the irritant is during the waking hours light, and during sleep a peculiar irritant unknown cause. (See § i.) The oculo-muscular nerve accordingly remains, in a normal state, at most times under the influence of several irritants, as well as the Sympathetic Nerve.

The Sympathetic Nerve, on the contrary, appears to be excited, in the normal state, only by some of the nerve-irritating agents above considered. In point of fact, it also appears that in all normal cases in which the pupil is dilated, the oculo-muscular nerve is found in a state in which it is in a less degree excited than usual; while a direct normal excitant of the Sympathetic Nerve appears not to exist. In this mode, for example, we must explain the dilatation of the pupil which takes place in twilight and that which occurs after natural death. It is observed, in short, very usually when death takes place, that the pupil is dilated, and subsequently resumes its normal, more narrow diameter. I found, in reference to this point, the diameter of the pupil to be, before death, seven millimetres; immediately after death, ten millimetres; and at a later period, only six millimetres. This phenomenon depends, as it appears to me, upon the circumstance that the cerebral nerves become lifeless sooner than the Sympathetic Nerve,—as I have already on another occasion shewn,—and that then the preponderating power of the Great Sympathetic may be manifested. The well-known fact, that after death the intestinal canal begins to act in a much greater extent and with more strength than during life ;—the rapid, sudden occurrence of the beat of the heart, which is occasionally observed in mammiferous animals immediately after death ;—the births which occasionally ensue after death, and similar phenomena ; all these I regard as arising from the same cause. In all I behold the evidence of organic equilibrium which is maintained by means of opposing power.

The same law which we see expressed in the organs of involuntary movement, the organs of what has been named Vegetative Life, on a small scale, and propagated from the central parts by the almost microscopical conducting fibres of the Nervous System, this same law appears more manifest, proportionally in the organs of the Animal Life. And the same opposition or antagonism which is evinced between the fibres which obey the will, and those which are regulated by the reflex force peculiar to the Spinal Marrow,—which are frequently met by physicians in cerebral palsies, and by physiologists in the instance of decapitations,—this same antagonism takes place also in the involuntarily moved organs ; and here it is on the one side the great Sympathetic Nerve, and on the other side, in the majority of organs belonging to this head, the Pneumogastric Nerve, as I shall study to prove at great length in a Treatise, which is afterwards to be published. In the case of the Iris, however, it is not the Pneumogastric Nerve which antagonizes or counterpoises the Sympathetic Nerve, but this function is represented by the Third and Fifth Cerebral Nerves.

If we recapitulate the representations hitherto given upon the relations of the Great Sympathetic Nerve, we may be permitted to say, that this nerve arises in the upper thoracic region from the Spinal Marrow, ascends upwards to the neck, and at length reaches the Ciliary Ganglion. Although I have now proved the origin of the Sympathetic Nerve from the Spinal Marrow, it is, however, not established whether fibres proceed not also from the ganglions which are situate between the spot of origin and the Iris ; so that consequently the Sympathetic Nerve may consist partly of peculiar filaments, partly of filaments from the Spinal Chord, and both of which may have influence upon the movements of the Iris.

We, that is Dr Waller and myself, had, on the 10th of

August 1851, cut through, in a rabbit, the Sympathetic Nerve on the right side; and six days afterwards we applied irritation to this nerve. No longer was any movement observed in the pupil. When, on the contrary, we applied the galvanic stimulus to the upper Cervical Ganglion itself, the pupil quite distinctly underwent dilatation. Two months afterwards, that is, on the 20th September,[1] we took away entirely the ganglion on the right side, and cut through the Sympathetic Nerve on the left side, yet without touching the ganglion. On the 7th of November, that is, nearly seven weeks later,[2] we put the animal to death by loss of blood. We immediately exposed the Spinal Marrow, an operation which was completed in some minutes, and applied irritation to the marrow, along the whole of that tract from which we formerly witnessed the influence upon the pupil. We could, however, neither upon the right pupil nor upon the left, observe the smallest change. As soon, on the other hand, as we applied irritation to the ganglion, which, as was observed, was left uninjured on the left side, the pupil was strongly and distinctly dilated; and on several repeated applications of the stimulus, the same result was exhibited.

In another rabbit we divided the two Sympathetic Nerves below the upper Cervical Ganglion, and then, when the animal was narcotized by means of ether, yet still living, we exposed the Spinal Marrow and applied irritation from the sixth dorsal vertebra to the Atlas. We could, however, remark no trace of Dilatation of the Pupil.

I further observed, in one rabbit, in which I had about two months and a half previously cut away a large portion of the Great Sympathetic Nerve, and in which no re-union had as yet followed, that irritation applied below the ganglion shewed no influence upon the dimensions of the Pupil, and that nevertheless, in consequence of irritation applied to the ganglion itself, and the trunk of the nerve lying above it, the Pupil underwent very manifest dilatation.

In a more recent Treatise, Dr Waller states that he had separated the superior Cervical Ganglion from all the nerves that are in connection with it, and cut through the great sympathetic below this ganglion; and some days afterwards he had beheld no action from the nerves below the ganglion, except dilatation of the pupil from the influence of the ganglion itself; on the contrary, no result from irritation of the nerves, of the upper division of the spinal marrow.

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[1] There is an inaccuracy in the statement of these dates by the author. Two months after the 10th or 16th of August would be the 10th or 16th October. not September. The statement, however, is given as it stands in the original.

[2] The same remark regarding the incorrectness of the dates is here applicable as above. The inaccuracy does not affect the validity of the experiment.

In December 1851, I had, along with a pupil, M. Jouck, performed the same experiment; and allowed the animal to live for two months and a half. In this instance also Dilatation of the Pupil was manifested after irritation of the Ganglion itself; but none from that of the portion lying below the Ganglion.

We have further several times performed, in common, experiments of irritation with the different cerebral nerves; but from none of these nerves have we been able to produce any Dilatation of the Pupil.

All these observations appear to declare in favour of a self-existent or independent condition of the first Cervical Ganglion, and consequently for a self-existent and independent character of all the ganglions. I am, nevertheless, unable to derive from these experiments a complete proof of the truth of the influence, and I must still consider the question as open to further inquiry. The most undeniable conclusion from the experiment is this, that the upper Cervical Ganglion possesses a more exalted degree of dignity than the Sympathetic Nerve below this ganglion. So far as I can comprehend the subject, we may conceive four possible cases by means of which the phenomena may be explained.

1. Conjecture First. The superior Cervical Ganglion may act as one central Organ, capable of reflex action; that is to say, Centripetal fibres enter its substance, and centrifugal fibres emerge from its substance. In this supposition, the portion of the sympathetic nerve situate below the Ganglion must be analogous to the posterior roots of the Spinal Marrow; and the portion situate above this ganglion must be analogous to the anterior roots. I hence infer that the lower Sympathetic Nerve is entirely void of sensation, as we can conceive a centripetal conduction without sensation. Such conduction, however, as it appears to me, always implies a periphery from which an excitant agent may act. This we in vain seek here in an unconstrained manner, because it must be conceived, with the highest degree of probability, that the inferior cervical ganglion acts exactly as the superior one, and that accordingly on this supposition, the entire portion of the sympathetic nerve between the two ganglions has partly a centrifugal, partly a centripetal direction; that fibres of both kinds must take their origin from the same inferior ganglion. It would be then further conformable to analogy to conjecture, that the fibres between the lower cervical gan-

glion and the Spinal Marrow, were also organs partly centripetal, partly centrifugal, in relation to the posterior roots of the first thoracic nerves. I cannot demonstrate that such a condition is not possible; but I must confess that it is difficult for me to regard this complication as probable, so long as a simpler mode of explaining the known processes is presented.

2. Conjecture Second. From the ganglion new motific fibres arise for the movement of the Iris. This appears to me not probable, for this reason, that it is admitted that the Gasserian ganglion and the ciliary ganglion are placed in the same condition. If, however, from the last-named parts, proceed fibres which have this same signification, as the supposed signification of the upper cervical ganglion, then, after extirpation of the upper cervical ganglion, irritation applied to the branch of the Trigeminal nerve ought still to induce dilatation of the Pupil. Our experiments, on the contrary, have shewn that this is not the case; it is rather the fact, that irritation no longer acts upon the whole remaining portion of the sympathetic nerve, when the upper cervical ganglion is taken away.

(Here I must further remark, that we applied irritation, not to the ciliary nerve itself, but only to the frontal branch close to its entrance into the superior orbital fissure; consequently an exact result in reference to the ciliary ganglion itself is still wanting.)

3. Conjecture Third. The gangliar globules which are taken up by the primitive nerve fibres, secrete Nervous Fluid, and are therefore in a condition to retain irritability throughout a long time, though they confer no other peculiarities belonging to the central organs. Thus, for example, after cutting through the Sympathetic Nerve below the Ganglion, the pupil is still narrowed, and even not in a smaller degree than in the case of the simultaneous extirpation of the Ganglion itself. In this hypothesis, it is remarkable, that the gangliar globules of the Gasserian Ganglion are not in the same condition; that is to say, they should secrete nervous fluid.

4. Conjecture Fourth. There exists still one Centre in the Spinal Marrow, at a considerable height, corresponding to the uppermost Cervical Vertebræ, similar, as a lower Centre, corresponding to the upper Dorsal Vertebræ. This supposition, which theoretically has considerable probability in its favour, has practically hitherto everything against it; by experiments it is expressly contradicted. I cannot refrain from observing, however, a circumstance which impairs the force of these experiments, and renders them less certain

than at first sight they appear to be. The operations are usually accompanied with very great hæmorrhage, and they are often followed rapidly, if not by death, at least by enormous diminution of Irritable Power; which is so much more the case when the animal has been previously subjected to the stupifying influence of ether. Immediately after death, the disappearance of Irritable Power is often so speedy, that the difficulty is thereby augmented. It is hence my belief, that before it is possible to form conclusions upon this point, it will be requisite to perform a very great number of experiments upon the Irritable Power, and in so doing, that each nerve must be isolated, placed upon a glass rod, when the irritating agent is applied; and that this must be done with the *Nervus Trochlearis*, the *N. Abducens*, the facial nerve, the Glossopharyngeal, the Pneumogastric, the Accessory, the Hypoglossal; the first, second, and third cervical pairs; respectively at their points of emergence from the brain and the spinal marrow; namely, the *Foramina Intervertebralia*.

It is hitherto only by physiological experiments that the inquiry has been conducted to determine the origin of the sympathetic nerve, and the direction through which it regulates the movement of the radiated fibres of the Iris. I now proceed to consider the results of microscopical investigations, which have been made with the same object.

Dr H. Nasse is the first person who, after having divided the nerves, has subjected to microscopical examination the upper portion, while retaining its connection with the central part and the peripheral ends. For this purpose, he employed only the ischiadic nerve, and this in frogs and rabbits. The results, which possess interest in relation to the present inquiry, he states in the following manner.[1]

The changes which the nerve, after removal of its connection with the spinal marrow, undergoes, consist in a separation of the primary fibres. These lose at first their cylindrical aspect, acquire transverse stripes, from which they appear to be divided into clear, small, more or less irregularly cylindrical portions, the height of which approaches nearly the diameter of the fibres. These transverse lines are produced by intercrossing of the fibres, while the sheathing wall being drawn forcibly inwards is confined to one small fold, so that the observer does not perceive externally the spot of the contraction. Secondly, there are formed in the nerves, small fat-globules of the disintegrated medullary matter, by which the fibre is rendered darker and less transparent. At a subsequent period the small fat-globules are united to larger drops, which, however, are still microscopical drops; then the sheathing wall of the nervous tubules gradually wastes away. It is remarkable, that the fat-globules are found more abundantly in the central end of the lower portion of nerve than towards the periphery.

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[1] Muller's Archiv fur Anatomie, Physiologie und, s. w. 1839, p. 405.

This description is derived from the experiments of Dr Nasse on Frogs. In rabbits, also, he found the same sort of degeneration still more strongly marked, in consequence of which the nervous fibres acquire an aspect like cellular tissue fibres, the content of which was entirely converted into fat-granules, which were deposited partly individually, partly in rows beside each other. Nasse is of opinion, that when this disintegration of fibres, and the loss of oily content is the consequence, the restoration of the function must be very difficult, though possible. In the central end of the divided nerve, Dr Nasse observed only the alteration, that it appeared to be broader than normally.

M. Valentin has, at the same time as Dr Nasse, made observations upon the condition of the nerves after division; but he has not directed his particular attention to the difference in the central and peripheral portions.[1]

MM. Günther and Schön have, on the other hand, prosecuted this subject still farther, and Stannius has directed attention to the Degeneration.[2]

I had, previous to 1851, at no time given attention to the subject, and I witnessed the Degeneration for the first time through Dr Waller, who, in the spring of this year, came to Bonn, and shewed me the degenerated Nerve-Fibres.

Dr Waller has recently been very fully occupied with the microscopical changes which take place in the peripheral portion of the division of the nerves, and has perfectly established the existence of those changes which his predecessors have discovered. He has observed the disintegration in the nerves, even to the ultimate ends, and has ascertained several individual stages in the alteration. He has, at the same time, given expression to the idea,[3] that the observer may ascertain the course of a nerve, if he cuts it through and traces the disorganized branches. This method of experimenting possesses the greatest value for physiological purposes, as to the course of the Great Sympathetic Nerve, but is much less useful as to the Cerebro-spinal Nerves; because, in the physiological point of view, for instance, no more proof is required that the Ischiadic Nerve is directed *from* the Spinal Marrow towards the extremity, and not the reverse. On the other hand, in the instance of the Great Sympathetic Nerve, the case is entirely different. To ascertain whether the Great Sympathetic proceeds towards the Cerebral Nerves, or descends downwards from the Brain, is a point which, as has been already stated, has been the subject of discussion by physiologists from the time of Petit. By means of the experiments performed in common by us, it has been ascertained also, in the anatomical manner, that the Sympathetic Nerve actually, as Petit assumed, ascends upwards by the neck to the head, and does not move in the inverse direction. Further we have seen in dogs, in which animals the united Pneumogastric and Sympathetic Nerves had a considerable time previously been divided, that in the portion lying below the section, the Pneumogastric Nerve was degenerated, the Sympathetic Nerve retained its fibres in soundness, while it was the reverse above the point of section. From this it consequently follows, that the portion of the Sympathetic Nerve above the point of section was no longer in connection with its centre. Already, three-quarters of a year previously, had Dr Waller alone observed, that the superior portion of the Sympathetic Nerve had after division become degenerated, had, nevertheless, not applied this observation to the course of the Sympathetic, and had not thence deduced the conclusion, which is mentioned. This fact hence acquired its actual physiologieal value, first by means of our conjoined labours, as is very clearly stated also in the *Comptes Rendus* for 1851, October the 6th, in a treatise prepared by Dr Waller, and subscribed by both of us.[1]

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[1] Valentin de Functionibus Nervorum, p. 162.

[2] Muller's Archiv. 1840. [3] Comptes Rendus, 1851, 1st December.

§ III. ON THE INFLUENCE OF THE TRIFACIAL NERVE, OR FIFTH PAIR, UPON THE IRIS.

Inflammatory attacks and injuries of the Iris in man have shewn that this organ possesses sensibility; and animals, in which the Iris has been subjected to mechanical irritation, give manifest evidence of suffering pain. Since, however, among the three nerves which go to the Iris, neither the Great Sympathetic nor the Oculo-muscular Nerve are sensitive, the source of sensation must be sought for in the Trifacial Nerve, which is distinguished for its great sensibility.

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[1] As a controversy on the right of priority upon this subject has arisen between Dr Waller and myself, I recommend the reader who feels interest in this question to a Tract upon this subject, which appeared at Bonn in 1852, and which is to be got at the establishment of Henry and Cohen.

When the Trifacial Nerve is paralysed, the sensation of the Iris indeed also ceases. The palsy of the Trifacial Nerve may be artificially produced in two modes; namely, first, by cutting through the nerve within the cavity of the cranium; secondly, by dividing one-half of the *Medulla Oblongata*. The first experiment was first performed by Fodera, then by Magendie; the second was performed by Magendie. The Trifacial Nerve arises, it will be remembered, at a greater depth than in the *Calamus Scriptorius;* and if, accordingly, the half of the *Medulla Oblongata* is cut through between the occipital bone and the Atlas, all the parts lying on the corresponding side of the face are completely deprived of sensibility. After this operation the eye becomes as fixed as if it were made of glass. The power of contraction is entirely gone. The operator may thrust a needle into the eye and wound the Iris, without any reaction taking place.

[I always prefer performing this experiment on Frogs, because in these animals the insensibility of the protruding eye is very manifest. On the opposite side, on which no operation has been performed, the eyelids immediately shrink on being touched.]

On the other side of the body, also, sensation is smaller in consequence of this operation. But the difference, nevertheless, appears sufficiently in the eyes.

[Further, sensibility is in the spine, on the side which has been operated on, stronger than on the side on which no operation has been performed, especially on the anterior or atlantal extremities. On the other side, frequently, no reaction takes place, or it takes place only at the beginning of the application of the stimulus. Palsy in movement, on the other hand, is on the side on which the operation has been performed; the animal falls on this side, and is uncommonly restless if he is placed on the opposite side, while he rolls over to the former. This rotatory movement also usually takes place spontaneously, immediately after the completion of the operation.]

The phenomena in Sensation are the same when the operator divides the Trifacial Nerve.

The object or purpose which the sensitiveness of the Iris has, it is not difficult to divine. The fibres of the Trifacial Nerve which are expanded in the Iris have to perform the same function as the sensiferous fibres in all muscles in general. By means of these fibres the state of weariness and of activity, the measure of the degree of Force present, and the exhaustion in the muscles, become known, and thereby the stimulus is excited to commence or to terminate movements. All the specified conditions are associated with changes in the contraction and elasticity of the muscular fibres; and these changes must be those which are affected by the sensiferous nerves, and upon which, through the medium of these nerves, is established (so to speak) the relation to the Central organ. Such also is the case with the Iris. The rays of light affect the optic nerve, and mediately also the oculo-muscular nerve. In this nervous chain, so far as we can conclude from experiments, there is contained no fibre which possesses feeling; (the optic nerve is sensational, but not feeling). The more the optic nerve is excited,—that is to say, the greater is the intensity of the light,—the more energetic is the oculo-muscular nerve; that is to say, the more completely the pupil is contracted. The contraction of the pupil consequently in this case is the measure of the force, by which the stimulus acts on the optic nerve. Of this contraction or narrowing of the Pupil, the Trifacial Nerve becomes sensible or percipient, in the same manner as the fibres of the Tibial Nerve, distributed to the Gastroknemii Muscles, become sensible of the often-repeated contractions in continued walking. Pain arises; and the pain felt in the Iris admonishes, as it were, that the irritation is too strong, and rouses the stimulus or impulse to withdraw the eye from the irritation.

If this nerve did not exist, the individual would expose the eye to the most intense rays of light, might look for the whole length of a day through magnifying glasses, and thereby might lay upon one of the most important organs of the body a heavier load than it was able to endure, and thus cause its destruction.

[It is to be here observed, that the reasoning of the learned Professor Budge, like many other kinds of reasoning, is in a great degree contradicted by facts of daily occurrence. Notwithstanding this inspecting, regulating, and warning power which Professor Budge ascribes to the Fifth Pair of Nerves, it is a certain fact, that the eye is not only hourly and daily exposed to the stimulus of intense light, more intense than it is fitted to endure, but for longer periods than it endures or can endure with impunity. Those who are in the habit of seeing various instances of disorders of the Eye and Vision, are well aware, that many instances of disordered vision, enfeebled vision, and actual loss of vision, are induced, not only by exposing the eye to intense degrees of light, and by the too long-continued use of magnifying glasses, by the use of microscopes, telescopes, by long-continued writing, and sewing of fine articles, but above all, by the liberal use of gas-light of high degrees of brilliancy, and by working with gas-light during the nocturnal hours. Where, then, was this inspecting, protecting, and regulating power of the Fifth Pair? The answer must be, that it was exactly where it always is; but that in the great majority of, it may be said with truth in all, the instances, it does not act or exert this power, until it is too late to prevent mischief from being done to the organ.

No fact is more clearly established, in tracing the steps by which organs and textures become injured by physical agents, and their functions impaired and finally annihilated, than this, that the irritations or stimuli of these agents act for a long time and repeatedly, without apparently producing sensible mischief; but, after the lapse of some time of this bad usage, the organ begins to shew that it has been injured, and when the injury is once commenced, it proceeds, and it is often extremely difficult, sometimes impossible, to stay its progress, until the organ is rendered entirely useless.

The lesion resulting from the action of these stimuli is so gradually, and by such minute steps, induced, that it is never perceived until almost irreparable mischief is completed. If the Fifth Pair or any other nerve really possess this power of shewing when any given organ is over-stimulated, either it has no means of making the fact known, or to its warnings no attention is given.

It is not different with other organs and their nervous branches. If Professor Budge actually believes, as he seems to do, that to certain nerves is assigned this function of ascertaining the internal sensations of organs, and announcing to the Brain the fact, that these organs are overworked and over-stimulated, it becomes his duty to establish this doctrine upon solid and unquestionable foundations. It is possible that he may be in the right, and there are several facts which may be interpreted in favour of this speculation; but in the meantime there are quite as many on the opposite side; and a most extensive collection of facts will require to be made and estimated, before it will be even safe to say, that the doctrine has a certain degree of probability.

It may be admitted that, within certain limits, the doctrine possesses some degree of probability; and the instance of the Iris as a regulator of light is one of the most appropriate proofs of its limited application. But take the instance of the stomach, the liver, the kidneys, the bloodvessels in general, and we find that the doctrine admits of most limited application. Every day several times the stomach is overexcited by articles of food and drink, both in quantity and quality; do the nerves of that organ in all cases indicate this in time to prevent bad effects? It requires, on the contrary, it is well known, a great deal of watching on the part of the individual, and great self-denial in many instances, to prevent various articles of food and drink from being taken, which, though inducing no uneasy feelings at the time, and on the contrary giving rise to comfortable if not pleasurable sensations, yet certainly, in three, five, or six hours, are the cause of most painful sensations. This is only one instance of the limited application of the doctrine; others it would not be difficult to adduce. But we add no more.

It seems therefore very doubtful whether the statements and inferences thus confidently made by Professor Budge, as to the protecting and regulating power of the Fifth Pair being so complete, can be at present admitted as facts of physiological science.]

It cannot be doubted, that if the conception here given be well founded, the Trifacial Nerve also feels or ascertains that condition of the Iris in which it is no longer contracted by its circular fibres, exactly as it ascertains similar conditions from other muscular Nerves.

Upon the ulterior mechanism, meanwhile, which follows this feeling or sensation which is conveyed by the Trifacial Nerve, experiments furnish no inferences, because we have seen neither a dilatation nor contraction of the Pupil ever arise, when we applied irritation to the central end of the divided Trigeminal Nerve.

So far upon the Sensation or Feeling power. But further, the movement of the Iris is not independent of the Trifacial Nerve. In rabbits, the Pupil becomes considerably narrower after division of this nerve within the cavity of the skull, as has been seen by many observers. The narrowing is longer in being accomplished than upon irritation of the Oculo-muscular Nerve; several minutes elapse before it attains its greatest degree. The narrowing also remains a long time, and only disappears, as M. Longet justly represents, after some days, when the Cornea is already become opaque.

After transverse section of one-half of the *Medulla Oblongata*, the Pupil in like manner contracts, as already was observed by Johann Müller in frogs, and by ourselves upon rabbits. We remarked that often it continues forty minutes or longer, before the contraction is gone. This observation I have rendered particularly manifest upon Frogs. Three days after that I had cut through the *Medulla Oblongata*, the Pupil, which had previously been very forcibly contracted, was again like that of the other side. We have in Rabbits divided first the Oculo-muscular Nerve, and afterwards the half of the *Medulla Oblongata* or the Trifacial Nerve; and nevertheless the Pupil underwent exactly as strong contraction as when the Oculo-muscular Nerve was left uninjured; so that we may with all certainty draw the conclusion, that this action proceeds not from the third pair of Cerebral Nerves.

This contraction of the Pupil which is manifested after division of the Fifth or Trifacial Pair of Nerves, we have often formerly produced by a simple compression of the Ophthalmic branch of that Nerve. After death, the phenomenon disappears very rapidly. It is very rare that it can be satisfactorily seen only a few minutes after death. Thus, for example, the masticating muscles in the head of the person, who suffered death by the guillotine, contracted very strongly, on applying a stimulus to the trunk of the Trifacial Nerve, while the dimensions of the Pupil remained unaltered. The Oculo-muscular Nerve, on the contrary, still manifested its complete influence over the Pupil.

In what manner we ought to explain so energetic a contraction of the Pupil, after the operation mentioned, it is not possible with certainty to say; it must hence at least be of importance to anticipate all the possible cases; and it must be left to further researches to determine what explanation is the correct one. Three cases present themselves for consideration. First supposition: With the first branch of the Fifth Pair or Trifacial Nerve there are mingled motiferous fibres, which accompany this nerve from its origin. Second supposition: The motiferous fibres arise from the Gasserian Ganglion, and the fibres, the stimulation of which causes contraction of the Pupil, run in a centripetal direction towards the Ganglion, and are analogous to the Sensitive Fibres. Third supposition: There is an antagonistic action, and it arises at the same stage with the contraction of the Pupil, after division of the Sympathetic Nerve, and removal of the Central portion of the Sympathetic Nerve of the Iris, in the Spinal Marrow.

From the observations which are given in the preceding and subsequent sections, it is meanwhile very probable that with the fibres of the Ophthalmic Branch, there are mingled motiferous fibres which belong to the Trifacial Nerve itself at its roots.

§ IV. ANTAGONISM IN THE MOVEMENTS OF THE IRIS.

Under this head are reckoned principally two Phenomena; namely, the first, that which causes the contraction of the Pupil, when the Sympathetic Nerve is divided in the neck;

and, secondly, that which causes dilatation of the Pupil after the division of the oculo-muscular nerve.

1. OF THE CONTRACTION OF THE PUPIL AFTER DIVISION OF THE SYMPATHETIC NERVE.

This observation derives its origin, as has been already mentioned, from Petit, and it has since received confirmation from many inquirers, particularly in dogs, cats, and rabbits. After the section of this nerve, as was already stated by Petit, the third eyelid in these animals is drawn over a part of the Cornea and covers it.

I have found that we can perform the experiment of Petit very well on Frogs, and thereby the result appears as striking as possible. For this purpose, by means of an incision made above the edge of the Scapula, I search for the emergence of the Pneumogastric Nerve from the cavity of the skull, where lies the strong yellow Ganglion of the Pneumogastric Nerve. The operator can here easily seize it by the forceps and divide the nerve. After the lapse of from one hour to one hour and a half, the pupil on the operated side has become considerably smaller, and the third eyelid is more expanded. This expansion, however, does not remain permanent, but the eyelid recovers in time its usual position.

This experiment in the Frog furnishes a particular opportunity to shew that the Sympathetic Nerve arises from the Spinal Marrow. I have found, namely, that when in a Frog we divide the second brachial nerve at the point of its emergence from the Spinal Marrow, and cut through both the posterior and the anterior roots, after the lapse of some hours, the pupil of the side on which the nerve has been divided is considerably narrower than that on the opposite side. This is still more manifest on the subsequent days.

When, on the contrary, the operator has divided only the posterior roots, and has left the anterior roots contracted, the pupil indeed undergoes contraction also in many instances; but this is not constant; it diminishes normally even on the first day, and the contraction is not so active as in the other Frog, in which both roots were divided. I have three living Frogs, in which the posterior root of the brachial nerve has been divided; in two of these we observe no contraction; in the third, only a very small degree of contraction.

Besides the second brachial nerve, the third also acts upon the Pupil; and its relations in reference to the posterior and anterior roots are quite similar as in the second nerve.

Hence, accordingly, the inference decidedly follows, that the Sympathetic Nerve proceeds through the anterior unganglioned root; consequently arises not from the ganglion, but from the Spinal Marrow itself.

Even for persons little practised in experiments it is easy to be satisfied in Frogs of the correctness of this fact, as the experiment is very simple. I performed it in the following manner. In a Frog which has been previously narcotized by means of Ether, I make on the median line of the skin of the back an extensive longitudinal incision, remove the muscles which lie below the inner edges,—that is the vertebral margins of the shoulder-blades, in the region of the second and third dorsal vertebræ,—break off these bones by means of nippers, remove membranes, calcareous matter, and vessels, and push forward, if only the hinder root of the brachial nerves is to be divided,—under this thick root, as soon as it comes into view, the point of a cataract-knife, until the same is cut off. I do the same with the posterior root of the third brachial nerve. If the anterior root is also to be cut through, we place the spinal marrow at this spot, after division of the posterior root, gently a little upon the side, and in like manner cut through with the cataract-knife the anterior root as it presents itself.

Another advantage which this experiment upon Frogs promises to furnish, consists in this, that these animals remain long in life, and consequently the operator can place a whole collection of them in one small apartment, in which he can perceive at a glance the effect of the different operations. I have, for example, in seven glasses one Frog with the entire half of the spinal marrow which rises behind the third pair of cervical nerves extirpated; the Pupils are alike; one Frog with the posterior second nerve cut; one with the posterior third nerve cut; one with the posterior and anterior second root; one with the posterior and anterior roots of the second and third nerves; one in which the sympathetic nerve has been divided.

The contraction of the pupil after division of the Sympathetic Nerve cannot have its origin in the nerve itself, but must be caused by the state of the Spinal Marrow. The reason for this opinion is, that the phenomenon follows in a very short time after the division. For instance, in dogs and black rabbits it begins in from a quarter to half a minute afterwards; so that a change in the state of the nerve cannot be conceived. It takes place also when the operator leaves the nerve entirely untouched, and only takes away the definite portion of the Spinal Marrow, as I have seen along with Dr Waller, both in Mammiferous animals and also in Frogs, in which a piece of the Spinal Marrow in the neighbourhood of the brachial nerves has been extirpated. The Ganglions also, as follows from this fact, do not excite this sort of antagonistic action, because the contraction is not wanting, although the sympathetic nerve remains in connection with all its ganglions.

If, accordingly, we are to seek in the Spinal Marrow for the power thus acting, the question may be proposed, how are we to represent this power to the mind? We may imagine, that a constantly-acting energy in the Spinal Marrow effects a continuous contraction of the radiated fibres of the Iris, and that an incessant energy in the brain is the agent of a continuous contraction of the circular fibres; and meanwhile both powers act in opposite directions; if the one upholds the other, the middle degree of the size of the Pupil is the result, until a stimulus at this point or at that induces a change. If I am not deceived, the former idea is the one most correct, though it is not exactly so expressed. This idea is nevertheless quite hypothetical, and no direct observations, so far as I am aware, are in its favour.

I would first inquire, whether the Antagonism here under consideration has any necessary communication between the central parts; whether, accordingly, contraction of the Pupil can first take place, when the change in the Spinal Marrow was or was not communicated to the Brain through the nervous connection. I divided in a rabbit the cervical Spinal Marrow to the bottom. [This operation is easily performed, without breaking through any of the vertebræ, as between the individual bones it is allowed to penetrate completely, and effect the perfect division by means of scissors.] The Sympathetic Nerve was also then divided on one side. No long time elapsed when the pupil of the corresponding side was considerably narrower than the opposite one. In this instance, accordingly, a communication of both central parts in relation to the Iris could not take place; nevertheless the antagonistic action followed. This action accordingly ought to proceed from the Spinal Marrow only; which, to express it materially, may be intuitively understood,—that we admit, that the fibres of the Sympathetic Nerve, when separated from the Spinal Marrow, no longer receive their nervous fluid, or, as the ancient writers express themselves, their nervous spirit from their central organ,—are hence no longer excited, as takes place in the normal condition. These fibres are overtaken by a dormant state. To this extent we may indeed give the theory another expression; but we can oppose to it nothing important or essential, as it appears to me.

It has been further conjectured, that in consequence of this dormant state, the antagonistic nerves, which effect the contraction and closing of the pupil, have the preponderance, and that hence the action of the division of the Sympathetic Nerve is to be explained. As, however, this conjecture is supported by no observations, I could not on this subject acquire greater certainty. If this theory were correct, then after the division of the Nerves causing the contraction of the pupil on the one side, and the Sympathetic Nerve on the other, no change in the pupil ought to take place, because both antagonistic powers are removed. I have in the rabbit separated the Optic Nerve, the Oculo-muscular Nerve, and the Sympathetic Nerve from their centres on the same side of the body; but I could not perceive that the Pupil on this side was different from that on the other. In this, however, the objection ought to be made, that the operation which is requisite for the division of the cerebral nerves, is not only every time attended with profuse hæmorrhage, but the Brain itself, even with the most delicate handling, is injured, and from the intercrossing of the cerebral nerves, the complication is augmented.

I have therefore selected Frogs, by which were obtained results, which I did not foresee. I separated first in a Frog on one side, the optic nerve and the oculo-muscular nerve; and extirpated then on the same side of the body, the portion of the spinal chord corresponding to the second and third pairs of nerves. Some hours afterwards, the Pupil on the operated side was considerably narrowed, so that accordingly the division of these two nerves was entirely void of influence. As, however, it might be imagined, that probably the Sympathetic Nerve possesses a greater counter-operation than the oculo-muscular nerve, I took away on the one side the optic nerve and the oculo-muscular nerve, and extirpated the specified portion of spinal marrow on both sides, in order to see whether on that side, on which the optic nerve and the oculo-muscular were divided, the dimensions of the Pupil were otherwise than on the opposite side. I and other persons who beheld the Frog so operated on, were not capable of observing any difference. I came accordingly from this experiment to the conclusion, that in Frogs at least, the contraction of the Pupil after division of the Sympathetic Nerve depends not, as is generally believed, on the antagonistic action of the oculo muscular nerve.

There remain hence only two other suppositions; namely, first, that the Antagonism is produced by the Trifacial Nerve or Fifth Pair; and secondly, that the condition of rest is the contracted state of the pupil, which is overcome through the constant energy of the Sympathetic Nerve; that is to say, that the Sympathetic Nerve retains the Pupil in a state of permanent dilatation. As the determination of the first supposition presents great difficulties, I shall first advance whatever speaks in favour of the second.

It has been above remarked, that Messieurs Duval, Rochard, and Petit have observed that in man, after death, the pupil is strongly contracted. In Frogs, after death, the contraction is very intense. I have often observed that a diminution to the amount of three millimetres takes place, which for the eye of the Frog I may say, is a very great diminution. We might therefore be perfectly disposed to admit that, during life, the Sympathetic Nerve constantly exerts a very strong action upon the radiated fibres of the Iris; and that the Oculo-muscular Nerve enfeebles the force of the Sympathetic Nerve, only in consequence of a particular stimulus, namely, in the normal state, that of Light. I cannot, however, conceal from myself that it is unsatisfactory to admit, that the one nerve must be in a state of constant activity, and the other nerve not.

It appears therefore next requisite to prove the relation of the Trifacial Nerve to the Great Sympathetic. For this purpose naturally it is necessary, first that the Trifacial Nerve be paralysed, and then that the Great Sympathetic be cut through. In Mammiferous animals the division of the half of the *Medulla Oblongata* is a very serious operation, and it is, after the performance of it, not advisable to make further experiments upon the same animal. It is followed by the contraction of the Pupil more speedily than when the operator has divided the Sympathetic Nerve. The latter is also the case in the division of the Trifacial Nerve. On these grounds my experiments with Frogs only appeared to promise the hope of obtaining proper results. I divided the half of the *Medulla Oblongata*, and then in the region of the second and third Nerves I took away the whole Spinal Marrow, and united the wound by means of suture. There was consequently on the one side the Centre of the Sympathetic Nerve removed, that is, consequently, the Centre of the Nerve which causes dilatation of the Pupil, and on the other side, not only the centre of this nerve, but also the centre of the Trifacial Nerve, that is to say of the Nerve which causes contraction. After the lapse of some hours, the pupil of the

Eye, on the side on which both centres were taken away, was larger than that on the side on which only the centre for the Sympathetic Nerve was taken away. This phenomenon, however, was not permanent. Some time afterwards we could no longer observe any difference in the pupils of the two eyes. Both were much more contracted, to the extent even of one millimetre and a half. Before the operation they measured four millimetres. Two hours afterwards, the one Pupil on the side of which the *Medulla Oblongata* was divided, was three millimetres; the other Pupil, two millimetres and a half. After the space of five hours, both Pupils were two millimetres and a half.

I afterwards divided in one Frog the anterior and posterior roots of the second and third nerves, and at the same time the half of the *Medulla Oblongata,* after which the corresponding pupil became considerably contracted.

I am therefore unwilling as yet to express my opinion, whether a form of antagonism exists between the Trifacial Nerve (Fifth Pair) and the Sympathetic Nerve, but leave to further researches the determination of this point.

The contraction of the pupil which ensues on division of the Sympathetic Nerve, remains a very long time, as has been already seen by several observers. I possess, for example, a rabbit in which, twenty-two weeks previously, the right sympathetic nerve had been cut through, and in which the right pupil is still smaller than the left.

The contracted pupil is distinctly affected by the light of the sun, and, proportionally, scarcely less than the sound eye of the other side. Upon this point I have made experiments upon three rabbits. In one, upon which the operation had been performed on the right side one hundred and five days previously, the left pupil was in the shade five millimetres large, in the sun three millimetres and a half. In a second rabbit, in which the operation had been performed on the same side twenty-eight days previously, the left pupil in the shade was eight millimetres, in the light five millimetres and a half; the right pupil was in the shade six millimetres, in the sun four millimetres and a half. In a third rabbit, the operation had been performed on the same side twelve days previously; the left pupil in the shade was seven millimetres, in the light four millimetres and a half; the right pupil was in the shade six millimetres, in the sun five millimetres.

2. DILATATION OF THE PUPIL AFTER DIVISION OF THE OCULO-MUSCULAR NERVE.

Many inquirers report that after division of the Oculo-muscular Nerve, the Pupil is dilated of itself, and also that in palsies of this nerve in man the same phenomenon has been observed. I must, however, in accordance with truth, confess, that I never in animals could so justly convince myself of this, neither in the experiments which I performed along with Dr Waller, nor in those which I performed alone. I must remark, however, that I experimented only on frogs and rabbits. In one pigeon, in which along with the pupil, M. Von Soist, I made an incision into one of the *Corpora Quadrigemina* at its lower surface beneath the Optic Nerve, the Pupil of the opposite side was larger than that on the corresponding side. In another pigeon in which we incised from above the *Corpora Quadrigemina,* both pupils remained unaltered. I am, up to the present time, not in a position to give any opinion, from personal experiments, upon the antagonism, which ought to proceed from the Oculo-muscular Nerve.

§ V. LOCAL IRRITATION OF THE EYE.

By applying stimuli to the eye itself, the nerves as well as the muscles of the Iris are excited. It is not possible to irritate alone the radiating and the circular muscular fibres, and ever so little the nerves. The same irritating agent acts at once on both sorts of muscles and nerves. Hence it proceeds, that on different conditions of individual nerves depends a different consequence of irritation ; and therefore the results remain not alike.

If we apply an irritant agent to the eye of a healthy Frog, which has not been stupified by means of Ether, the Pupil undergoes contraction every time. This phenomenon is manifested in like manner under the same circumstances in rabbits, dogs, cats, and Birds, and appears consequently to be general.

Also immediately after death in dogs, cats, rabbits, Birds, and in the head of the person who suffered death by the guillotine, we observed the Pupil to be contracted in consequence of local irritation.

In rabbits, meanwhile, this phenomenon disappears very speedily after death. In dogs we saw it even after the eye had been extirpated.

So soon as in the Mammiferous animals in which we experimented, no contraction of the Pupil after death took place, there followed quickly dilatation, which at first went only after the direction of the wire, by which the pupil retained an oblong shape ; afterwards it became general.

In rabbits, in which several weeks previously the sympathetic nerve had been extirpated in the neck with the first cervical ganglion, we could no longer produce after death any dilatation by the use of irritant agents.

After division of one side of the *Medulla Oblongata*, there takes place on the corresponding side contraction of the Pupil, which after some time again spontaneously disappears. The sensitiveness of the Trifacial Nerve (Fifth Pair) of this side, is on the other hand, perfectly gone. If to the eye local irritation is applied, dilatation of the Pupil then takes place.

Lastly, the same phenomenon occurs, when in animals fully narcotized by ether, for which purpose Frogs are most suited, or when in Frogs after complete destruction of the Central part of the Nerves, the same experiment is performed.

Retrospect.

After death has taken place the Pupil is narrower than during life. During life the Sympathetic Nerve exerts in the state of rest, the strongest action upon the Iris. It is distributed upon the radiating Fibres of this organ, which radiating fibres are allowed to possess the preponderance over the circular Fibres. This Nerve regulates the dilatation of the Pupil. Its origin is the superior thoracic portion of the Spinal Marrow; and the fibres of the Sympathetic destined for the Iris principally go out with the first and second thoracic nerves, and form that nerve which proceeds from the last cervical ganglion to the first, then runs with the Carotid Plexus to the Gasserian Ganglion, lastly, with the ophthalmic branch of the Trifacial, or Fifth Pair, and with the long root of the Ciliary branch, joins the Ciliary Ganglion.

The condition of Dilatation is, as has been already said, the state of rest in the Iris during Life. This state is interrupted, when the Nerve which presides over the contraction of the Pupil, namely, the oculo-muscular nerve, comes into action. The usual excitation for the action of the Oculo-muscular Nerve is effected by the light acting upon the Optic Nerves. The connection between the Optic and the Oculo-muscular nerves is effected by the anterior pair of the *Corpora Quadrigemina*; [*Nates*].

Whenever the light becomes intense, the force of the Oculo-muscular nerve is strengthened, and the pupil becomes narrower. In twilight and darkness the stimulus disappears, and the Sympathetic Nerve, which represents the indifferent or neutral state, predominates. Immediately after death, first the Optic Nerve and the Oculo-muscular Nerve lose their irritable property, while the Sympathetic Nerve remains still excitable. Hence the pupil undergoes dilatation immediately after death.

The Trifacial Nerve or Fifth Pair is the sensitive Nerve. It is the sentinel or guardian over the Eye, and keeps watch to give warning when the stimulus is too powerful. It also accomplishes long-continued contraction when a stimulus is applied to it.

A. Contraction of the Pupil may take place in different modes.

1. Through the medium of the Oculo-muscular Nerve, when a stimulus is applied to it, which may be in the following modes.

a. By intense degree of light.

b. During sleep.

c. By involuntary, probably also morbid, turning of the eye inwards.

2. Through the medium of the Sympathetic Nerve, when this nerve in its whole above-described course is divided or strongly compressed.

2 a. By destruction of the Spinal Marrow in the region of the lower cervical and upper thoracic portion. This result hence becomes possible in *Tabes Dorsalis*, as has been already observed. See **Romberg**, Nervenkrankheiten I. p. 797.

3. Through the medium of the Trifacial Nerve or Fifth Pair. It is, however, not yet ascertained whether also in man this cause operates.

B. Dilatation of the Pupil may take place under the following circumstances.

1. From Irritation applied to the Sympathetic Nerve, and the Spinal Marrow at the spots indicated.

2. When the Oculo-muscular Nerve has lost its capability of being stimulated. This may be

a. From insensibility of the Retina and the Optic Nerve.

b. From destruction of the lower part of the anterior pair of *Corpora Quadrigemina*; (Nates):

c. From pressure or destruction of the Oculo-muscular Nerve itself.

Upon the Dilatation of the Pupil which takes place from the action of Belladonna, I have not hitherto extended my researches.

C. The pain in the Iris depends in every case on affections of the Trifacial Nerve.

ART. II.—*Observations on the Experiments of* Professor BUDGE *upon the Influence of the Great Sympathetic, with some notice of the Experiments of* M. CLAUDE BERNARD *upon the same subject.*

In the preceding article the whole of the essay of Professor Julius Budge is given entire, without retrenchment, addition, or comment, except in one instance in which a few words by way of note seemed to be requisite. It is unnecessary to express any opinion upon the merits of the facts adduced and reasonings presented by the author, as this has been already done by the Commission of the Academy of Medicine at Paris, and by other bodies. The purpose of the preceding article is to make generally known these facts and reasonings, and allow readers to draw their own conclusions.

Meanwhile it is to be observed, that M. Julius Budge, and his coadjutor Dr Waller, have not been able to escape the imputation of having overlooked and neglected, in this inquiry, certain researches which had been made by M. Claude Bernard, and which that gentleman regards as having previously determined several points in a more satisfactory manner than had been done by the German experimentalists. On the justice and merits of this counter-claim, we give no opinion. But it is proper to mention shortly the statements and allegations of M. Claude Bernard.

M. Cl. Bernard, who, on the 7th of March 1853, presented a Note to the Academy of Medicine, at their meeting of that date, on the *Multiplicity*, *of the Phenomena which result from the destruction of the Cervical Part of the Great Sympathetic Nerve,* maintains that Professor Budge and Dr Waller appeared to him to possess neither a complete knowledge of his labours, nor a just general understanding of the multiplied phenomena which attend on the division of the Great Sympathetic Nerve. For this reason he believed it to be his duty, to establish the facts in chronological order, and to shew the share which belongs to each inquirer in these researches. The Reporters of the Academy speak in highly eulogistic terms of the neatness and accuracy with which this was done by M. Claude Bernard.

He mentions first what is known to all, that the earliest known experiment upon the cephalic portion of the Great Sympathetic Nerve belongs to Pourfour de Petit, who, in 1727, shewed that the division of the cervical portion of the Great Sympathetic, besides some phenomena of injection of the side of the eye, uniformly induces contraction of the pupil of the corresponding side. The same phenomenon is produced, when, instead of cutting the Sympathetic trunk, we extirpate the superior or inferior cervical ganglion.

In 1846, Signor Biffi of Milan observed this new fact, that when the pupil undergoes contraction after the division of the Sympathetic Nerve, it is possible to restore its dilatation by applying galvanism to the cephalic end of the divided nerve.

Nearly at the same period, Dr Ruete, having remarked that in palsy of the Third Pair of Nerves, the pupil, dilated and immovable, may be still farther enlarged under the influence of Belladonna, thence deduced the inference, that the Iris receives two sorts of motiferous nerves, corresponding to its two orders of muscular fibres, and that the Great Sympathetic, by giving animation to the radiated muscular fibres, produces the movement of dilatation; while the common motor nerve, the oculo-muscular, by giving animation to the circular fibres, produces, on the contrary, the movement of contraction in the Iris.

In 1851, MM. Budge and Waller ascertained that, in its action upon the pupil, the cephalic filament of the Great Sympathetic Nerve only acts as a conductor of an influence, the point of departure of which is situate in a definite region of the Spinal Marrow, to which region they applied the name of *Cilio-spinal*, and which particularly gives origin to this cephalic portion of the Great Sympathetic.

It is requisite here to mention, that Professor Budge, in a letter addressed to M. Flourens, and which was read to the Academy at its meeting on the 28th of February 1852, states that he had found that there is in the Spinal Marrow a certain region, the extirpation of which increases greatly the heat of the head. This region is situate between the last cervical vertebra and the third dorsal vertebra, and this phenomenon is transmitted by the eighth cervical nerve, and the first and second dorsal nerves. After laying bare, in rabbits, the region of the spinal chord indicated, he removes one-half of it, beginning from the last cervical nerve to the third dorsal nerve. In the course of from ten to fifteen minutes, the heat of the ear of the same side becomes so much increased, that it is easy to distinguish the two ears by merely touching them. When the atmosphere is cold, the ears of rabbits are usually, at the tip, of the temperature of from 29° to 30° centigrade = to 85°-86° Fahrenheit, more or less. On the side on which the half of the spinal chord has been removed, the thermometer indicates from four to five degrees more than on the other side. The arteries throb, and the vessels are dilated.

M. Claude Bernard had observed the same effect after dividing the Sympathetic Nerve in the neck. Professor Budge finds that the Great Sympathetic Nerve is the medium by which the influence of the Spinal Marrow is transmitted. The origin of the Spinal Marrow here mentioned, adds M. Budge, is the same in which the fibres of the Great Sympathetic Nerves, which are distributed to the Iris, take their origin. As he found that the Sympathetic filaments of the Iris arise from anterior or motiferous roots of this part of the Spinal Marrow, he also observed the same result in relation to the development of heat; for, if he divided only the posterior, that is, the sensiferous roots, the heat of the head was either not altered, or it was so only in a very small degree.

M. Claude Bernard, in researches inserted, in the months of October and November 1852, in the *Comptes Rendus* of the Biological Society, recapitulates several of the general results of his experiments upon the Great Sympathetic Nerve; and he shews that the contraction of the pupil, to which physiologists had attached great importance, is far from being the only phenomenon which is produced after the destruction of the cephalic portion of the Great Sympathetic. In giving a generalized expression to this phenomenon, M. Bernard shews that this experiment is followed by very multiplied disorders, which are as follow.

1*st*, Contraction of the pupil; 2*d*, The irritation or diminution of the palpebral opening, and at the same time deformity of this opening, which becomes elliptical, and then transversely oblong; 3*d*, The retraction of the eyeball towards the bottom of the orbit,—a retraction which causes the third eyelid to protrude, and thereby to be pushed over the front of the eye; 4*th*, The contraction, more or less marked, of the nostril and of the mouth of the corresponding side; 5*thly and lastly*, A special modification of the circulation, coinciding with a great increase in the temperature and even in the sensibility of the parts.

All these phenomena, as well as that of the augmented calorification, M. Claude Bernard argues, proceed evidently from the state of the Spinal Marrow, since it has been ascertained that it is this part of the body which is the centre or origin for the Sympathetic Cervical Nervous Filament; and as the last-mentioned part is by itself only a mere conducting organ, M. Professor Budge in making this statement in his last communication to the academy, has, according to M.

Bernard, added nothing to the discovery of the increased calorification made by M. Cl. Bernard.

Dr Waller had made upon the question of augmented heat and increased vascular action additional researches. The object of Dr Waller was to shew that it is possible to regulate at will the phenomena which follow the division of the Great Sympathetic Nerve, so as to diminish or to increase, within certain limits, the activity of the circulation of the blood and the temperature of the neighbouring parts.

When we divide or inclose in a ligature the cervical portion of the Sympathetic Nerve, or subject it to the repeated stimulation of galvanism, the opening of the pupil is contracted, the *membrana nictitans* advances over the surface of the eyeball, and the vessels of the eye become more red. The temperature of these parts is increased to such a degree as to be easily estimated by the hand and by the thermometer. These phenomena continue in the same state for a very considerable time, ordinarily for several months. If we apply galvanism to the upper end of the nerve, the pupil is dilated to its greatest extent. The *membrana nictitans* is withdrawn into the angle of the eye; and at the same time if we examine the vessels of the conjunctiva and those of the ear, we perceive that they are empty, and that the skin and the mucous membrane return to their normal condition.

If then we withdraw the galvanic poles, the contraction of the pupil and the injection of the vessels soon reappear. Upon removing and approximating these poles alternately to the nerve, we make the ocular, vascular, and auricular phenomena appear and disappear alternately as we choose. While the injected vessels empty themselves of blood, the temperature of the skin falls; and Dr Waller was able in this manner to cause the thermometer to fall from one degree to one degree and a half of the centigrade scale.

The ocular and the auricular phenomena are closely connected together.

The action of the nerve is evinced almost exclusively upon the arteries and the capillary vessels. These vessels are contracted and at length obliterated, while the veins remain black and distended with blood. In a large rabbit, with pendulous ears, a variety common in England, towards the base of the ear, though the diameter of the artery was more than one line, the internal cavity became empty and completely disappeared under the action of galvanism. When the galvanic current ceased, the undulating blood appeared in the vessel.

The vascular turgescence produced by the action of topical

irritant substances, as very hot water, mustard, ammonia, disappears almost completely under the influence of galvanism. After division of the Great Sympathetic, the capillary hæmorrhage of the operated side is greatly more copious than that on the other side; this hæmorrhage diminishes or entirely ceases. If we galvanize the divided nerve, all these differences in animal temperature are especially remarkable in the cold season; for a temperature of fourteen degrees was required to establish the equilibrium.

In the month of February, when the temperature was below zero cent., the ear of the operated side presented a temperature of ten degrees above the unoperated side. This great inequality disappeared gradually, when the animal was placed in a medium with a higher temperature. At this time Dr Waller found in dogs the temperature of the ear of the operated side ten degrees cent. above that of the side not operated on, the latter being twenty-five degrees, = 77° F., while the former was thirty-five degrees, = 95° F. After the animal had remained half an hour in the chamber in which the operation was performed, where the temperature was at fourteen degrees cent., = 58° F., the unoperated side indicated a temperature of twenty-nine degrees, = 85°, while the side on which the operation was performed remained unchanged. After one hour the unoperated side was at thirty-four degrees, the operated side at thirty-five degrees cent.; and at these points the temperature of both sides remained stationary.

The inclosure of the artery in a ligature, or the inclosure of the jugular vein, appears not to affect the temperature of the ear of the operated side; but upon applying galvanism to the lower end of the pneumogastric nerve, divided so as to weaken greatly the action of the heart, a depression of one degree of the centigrade scale takes place.

The arterial contraction is equally produced by irritating by galvanism the Great Sympathetic Nerve in every part of the cervical region. The centre of this nervous action is found in the Spinal Marrow. The maximum effect is produced by galvanizing the spinal marrow at the level of the articulation of the second and third dorsal vertebræ.

After division of the Cervical Sympathetic Nerve, even after applying galvanism to it several times, the part of the nerve which is above, is disorganized. Further, at the end of four or five days, galvanism applied to this chord from the point at which it has been divided, or galvanically stimulated, to the first cervical ganglion, produces no effect upon the vessels of the ear. But if at this time we apply galvanism to the first cervical ganglion, we produce at once constriction

in the Comptes Rendus for 29th March 1852, the following statements are given.

When in a Mammiferous animal, for instance a dog, a cat, a horse, or a rabbit, we divide in the middle region of the neck the nervous filament of communication which lies between the inferior cervical ganglion and the superior cervical ganglion, or remove entirely the cervical ganglion itself, we find immediately that the temperature increases in the whole corresponding side of the head of the animal. This increase in temperature commences in an instantaneous manner, and so rapidly is it developed, that in some minutes, and in certain circumstances, we find between the two sides of the head a difference which may occasionally rise to three or four degrees in the centrigade scale. Though this difference in temperature is readily perceived by the hand, it is more accurately and palpably demonstrated by introducing a small thermometer into the nostrils and the auditory tubes of the animal. This difference of from three to four degrees is remarkable as a difference of relative calorification between the two sides of the face. But then, if we compare the heat of the ear and of the nostril, thus increased in consequence of the division of the nerve, to the heat of the rectum, of the central parts of the body, the chest, and the abdomen, we perceive that it is nearly the same. M. Bernard ascertained sufficiently often that the division of the thread of the cervical Sympathetic Nerve in the rabbit raises in the corresponding ear the heat to forty degrees, while the normal temperature in the rectum in this animal generally exceeds not thirty-eight or thirty-nine degrees.

The whole of that part of the head which becomes hot after the division of the nerve, becomes the seat of a more active circulation of the blood. The arteries especially seem filled, and appear to throb more forcibly. But the subsequent days, sometimes even next day, this vascular turgescence has considerably diminished, or even has disappeared, though the heat of that side of the face continues to be as great as the previous day. This circumstance ought to make us think that the elevation of temperature is not solely owing to the greater activity of the circulation. As to other points, upon observing for a long time the animals which present this phenomenon—and M. Bernard observed it twelve or fifteen days in the rabbit, and several months in the dog—he never saw, after this experiment, the supervention in the hottest parts of either œdema or any other morbid phenomenon which could be ascribed to what is called Inflammation.

The facts here stated, Dr Waller thinks calculated to throw much light on various phenomena of living bodies.

The flow of blood into the vessels of the face, in consequence of mental emotions, the erection of the penis, of the nipple, the turgescence of the mucous membrane of the intestinal canal at times when digestion is not proceeding, and many other phenomena of the same kind, are to be explained, he thinks, by the existence of canals, the dimensions of which vary according to the influence of the Cephalo-rachidial axis. The contraction of these same vessels under the influence of cold, which several authors attribute merely to physical causes acting on elastic tubes, is not less under the dependence of the centres of innervation.[1]

M. Claude Bernard maintains that he performed all these experiments previous to their performance by Dr Waller.

To establish this claim of priority, he adduces from the Reports of the Society of Biology for November 1852, the following passage.

If we galvanize, says M. Bernard, the upper end of the divided Great Sympathetic Nerve all the phenomena which we have seen produced by the destruction of the Great Sympathetic change appearance and become opposite. The pupil becomes dilated, the palpebral opening is enlarged, the eye projects from the orbit. The circulation from being active becomes enfeebled; the conjunctiva, the nostrils, the ears, after having been red, become pale. If we stop the galvanism, all the phenomena originally produced by the destruction of the Great Sympathetic gradually return, and manifest their presence once more on a second application of galvanism. This experiment we may continue as we please, repeat as often as we wish; the results are always the same. If we apply a drop of hartshorn on the conjunctiva of a dog, on the side in which the Great Sympathetic has been destroyed, the pain resulting obliges the animal to keep the eye obstinately and firmly closed. At this instant, if we galvanize the superior end of the Great Sympathetic which has been divided, notwithstanding the pain which the dog feels, he can no longer keep the eye shut, the eyelids are largely opened, while the redness produced by the caustic hartshorn diminishes and almost entirely disappears.

Regarding the contraction of the arteries, M. Bernard has remarked this result, not only in the capillary vessels, but also in arteries of large diameter, and sometimes upon the carotid artery itself.

In a more ample account of his researches on this subject in the Comptes Rendus for 29th March 1852, the following statements are given.

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[1] Comptes Rendus, No. 9, Fevrier 1853. Tome xxxvi., p. 379.

When in a Mammiferous animal, for instance a dog, a cat, a horse, or a rabbit, we divide in the middle region of the neck the nervous filament of communication which lies between the inferior cervical ganglion and the superior cervical ganglion, or remove entirely the cervical ganglion itself, we find immediately that the temperature increases in the whole corresponding side of the head of the animal. This increase in temperature commences in an instantaneous manner, and so rapidly is it developed, that in some minutes, and in certain circumstances, we find between the two sides of the head a difference which may occasionally rise to three or four degrees in the centrigade scale. Though this difference in temperature is readily perceived by the hand, it is more accurately and palpably demonstrated by introducing a small thermometer into the nostrils and the auditory tubes of the animal. This difference of from three to four degrees is remarkable as a difference of relative calorification between the two sides of the face. But then, if we compare the heat of the ear and of the nostril, thus increased in consequence of the division of the nerve, to the heat of the rectum, of the central parts of the body, the chest, and the abdomen, we perceive that it is nearly the same. M. Bernard ascertained sufficiently often that the division of the thread of the cervical Sympathetic Nerve in the rabbit raises in the corresponding ear the heat to forty degrees, while the normal temperature in the rectum in this animal generally exceeds not thirty-eight or thirty-nine degrees.

The whole of that part of the head which becomes hot after the division of the nerve, becomes the seat of a more active circulation of the blood. The arteries especially seem filled, and appear to throb more forcibly. But the subsequent days, sometimes even next day, this vascular turgescence has considerably diminished, or even has disappeared, though the heat of that side of the face continues to be as great as the previous day. This circumstance ought to make us think that the elevation of temperature is not solely owing to the greater activity of the circulation. As to other points, upon observing for a long time the animals which present this phenomenon—and M. Bernard observed it twelve or fifteen days in the rabbit, and several months in the dog—he never saw, after this experiment, the supervention in the hottest parts of either œdema or any other morbid phenomenon which could be ascribed to what is called Inflammation.

M. Bernard wished to inquire what results would be presented by the head heated by the division of the Sympathetic Nerve, in comparison with other parts of the body, if the animals were exposed to great variations of surrounding temperature. He accordingly placed in a stove a rabbit in which the nerve had been divided, in a medium the temperature of which was above that of the animal's body. The side of the head, which was already hot, did not become sensibly hotter, while the opposite half of the face became heated; and very soon it was no longer possible to distinguish the side of the head on which the Sympathetic Nerve had been divided, because all the parts of the head, on acquiring their highest amount of caloric, were placed in harmony of temperature.

Very different was it when the animal was cooled by placing him in a medium the temperature of which was a good deal below that of his body. The part of the head corresponding to the divided Sympathetic Nerve resists much more the cold than that of the opposite side; that is to say, that side of the head not subjected to operation is cooled, and loses its temperature a great deal more quickly than that of the opposite side. So that then the want of harmony in temperature between the two halves of the head becomes more manifest; and in this circumstance it is possible to ascertain a difference in temperature, which may in some instances, as has been said, extend to three or four degrees in the centigrade scale.

This singular phenomenon of greater resistance to cold is accompanied also with a species of exaltation of the vitality of the parts, which becomes particularly manifest when the animals are made to die slowly, either by administering poison in a certain manner, or by dividing the two pneumogastric nerves. In proportion as the agony of death approaches, the temperature falls progressively in all the external parts of the body. But we always find that the side of the head in which the Sympathetic nerve has been divided presents a temperature comparatively more elevated; and when death takes place, it is the side of the face which longest retains the characters of life. At the moment, indeed, at which the animal ceases to live, there may be an instant at which the unoperated side of the head already presents the chill and immobility of death, while the other half of the face, or the side of the divided sympathetic nerve, is sensibly warmer, and still retains those sorts of involuntary movements which depend on sensibility without consciousness, and to which the name of reflex movements has been applied.

In conclusion, M. Bernard thinks that this experiment will certainly contribute to elucidate the functions of the Great Sympathetic Nerve, and proves that it exerts a very different influence upon animal heat when we operate on the great sympathetic nerve instead of acting on the nerves of the Spinal Marrow. He adds that these facts ought to form important elements in the complicated question of the nature and origin of animal heat, for the solution of which, as MM. Regnault and Reiset have observed, it is not sufficient to invoke the aid of the phenomena of respiration.

Against the claim above set forth, M. Professor Budge made a reclamation which was read in the Academy on the 28th of March 1853.

In this M. Claude Bernard replies, on the 4th April 1853, that he declines following M. Budge in his long discussion, which he says is foreign to the subject of the communication of 7th March 1853; that he had established the fact that in the researches of M. Budge and Waller upon the cephalic portion of the Great Sympathetic, addressed to the Academy on the 8th October 1851, they had, like their predecessors, seen only one phenomenon, the contraction of the pupil; that he (M. Bernard) had been the first who shewed that in the destruction of the cephalic portion of the Great Sympathetic, besides the circumstance of contraction of the pupil, there was that of calorification and several others then mentioned. He finally maintains the perfect correctness of the historical view which he delivered.[1]

In this position stands the controversy at the present time. The claimants may be allowed to settle the disputed points in their own way and at their own time. It may perhaps be allowed that there is some difference between the chord of the sympathetic nerve on the one hand, and the spinal chord on the other; and that in relation to the supposed influence of each of these parts upon the phenomena of animal heat, it is necessary to observe this distinction. But whether this be admitted or not, it is certain that long before the time either of Professor Budge or M. Claude Bernard, the general question of the influence of the nervous system upon the production of animal heat had been examined with great care and ingenuity by an English physiologist, whose experiments placed him at the time at the head of English physiologists, as his writings have since that given him the character of the first of English surgeons.

In 1811, forty-two years ago, Sir Benjamin Brodie published in the Transactions of the Royal Society the Croonian lecture for 1810, containing researches respecting the Influence of the Brain on the action of the Heart and the generation of animal heat; and in 1812 he published further experiments and observations on the Influence of the Brain in the generation of animal heat.

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[1] Comptes Rendus, 4 Avril 1853, No. 14. Tome xxxvi., p. 632.

In these experiments Sir Benjamin Brodie was led to conclude that the production of animal heat is in a great degree under the influence of the Nervous System, and that the temperature of warm-blooded animals, that is the Mammalia and Birds, is considerable under the influence of the Nervous system.

It had been shewn by Dupuytren, that in an animal in which both the pneumogastric nerves are divided, the blood returned from the lungs has a darker colour than natural, and that the animals on which this operation is performed die at various intervals afterwards, with symptoms of asphyxia, notwithstanding that air continues to enter the lungs; and he hence concluded that the changes which are produced on the blood by respiration are not the result of a mere chemical process; but are dependent on the nervous influence, so that they cease to take place when the communication between the lungs and the brain is destroyed.

Sir Benjamin Brodie was, on the other hand, of opinion that experiments shewed that the influence of the Nervous System does not appear to be necessary to the production of the chemical changes which the blood undergoes in consequence of exposure to the air in the lungs. In other words, Sir Benjamin Brodie thought that there is evidence to shew, that the influence of the Nervous System in maintaining and generating animal heat, is not exerted through the medium of the function of respiration, but acts in a more direct manner.

In his subsequent reasonings on this question, enlarged and modified as they are by subsequent experiments, especially those of M. Dulong, M. Dupretz, and, not long ago, those by MM. Regnault and Reiset, Sir Benjamin confesses that it is not the mere diminished consumption of oxygen that is sufficient to explain the lower temperature of animals in which the nervous system has sustained injury; but that whatever be the effect of the conversion of oxygen into carbonic acid, there must be some other source of animal heat.[1]

Shortly after the time at which Sir Benjamin Brodie first directed attention to this subject, M. Le Gallois had occasion to consider the influence of the Nervous System in the production and maintenance of animal heat, in his work on

[1] Additional Notes, p. 118.

the Principle of Life, published in 1812. M. Le Gallois observed the rates of cooling in animals in which the head was cut off, in animals in which the brain was destroyed, in animals in which the spinal marrow was destroyed or cut across, while the brain was left entire. The general result of these experiments, avoiding minute exceptions and discrepancies, is to shew that the Nervous System, that is the brain, through the medium of the Eighth Pair and Great Sympathetic, or the Spinal Chord, exerts a certain amount of influence in the production of animal heat, independent of the oxygen inhaled and the carbonic acid gas eliminated by the function of respiration.

The question, nevertheless, is one not only difficult, but complicated. Magendie shewed that after the recurrent nerves have been injured by dividing the eighth pair or the recurrent branches themselves, a peculiar lesion of the function of Respiration follows, partly from the effect upon the movements of the larynx. The dilating muscles of the *glottis* are thereby paralyzed; this aperture is no longer opened during inspiration; while the constrictive muscles which receive their nerves from the superior laryngeal chords retain their power, and consequently close more or less completely the *rima glottidis*. This is followed by a lesion in the bronchial tubes, the air-cells, and the substance of the lungs.[1] In the experiments performed by the late Dr John Reid, by dividing the eighth pair, it was a common result to observe the lungs so much inflamed that they were quite solidified, and sank in water like a stone. In almost all these experiments it has been observed that animals die of lesion of the lungs, a sort of slowly-approaching but very sure asphyxia; and it seems to be merely a question of time when this lesion is to be completed so far as to prove fatal.

It may be asked, is the cooling process quite independent of this lesion of the larynx and of the lungs?

Sir Benjamin Brodie seems to think, in his recent Notes on the subject, in mentioning the fact observed by M. Le Gallois, that living rabbits, merely from being confined in a restrained position on the back gradually lose temperature, even to the extent, if continued a considerable time, of causing the death of the animal; that one cause of the loss of heat in animals, either after operations of the kind referred to, or when placed in constrained positions, without operations having been performed,—is the state of alarm in which the animal is placed, operating upon him in the same manner as the same depressing passion operates on the human subject.[1]

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[1] Preçis Elementaire de Physiologie. Par F. Magendie, M. d'Institut de France. Deuxieme Edition, t. ii., p. 355, 356.

It is scarcely necessary to remark, when speaking of this subject, that there are two varieties, as they may be called, of animal heat; one a healthy natural heat, taking place in normal circumstances; the other a morbid heat, developed under unnatural and unhealthy conditions. Though this distinction may not be recognized by scientific inquirers, and is in general, indeed, overlooked, yet various facts shew that it is quite real, and that unless attention is given to it serious mistakes may arise. In the case of healthy, natural heat, the system seems to possess powers capable of regulating its degree, and never allowing it to become excessive, and either a cause of injury or an indication of lesion. The means by which this is effected are chiefly the skin, the lungs, and the kidneys. In the case of unnatural, morbid heat, it is totally deficient. In all instances it is a proof that something is wrong, either in the system at large, or in some particular part or organ.

The peculiar heat appearing in the side of the head of animals in which the Great Sympathetic has been divided or injured, or the cephalic portion of the Spinal Marrow on that side has been injured or removed belongs to the second form of animal heat. It is manifestly not natural or normal. It is one of the effects of a great lesion of the nervous system; and, whether it be regarded as the cause or the effect in the associated fact of the disordered state of the vascular system, it is manifest that it is to be regarded as an unhealthy and unnatural increase of temperature.

On the exact mode in which this lesion of the Nervous System acts in producing the increased heat, none of the expermentalists give any opinion. It may therefore seem presumptuous, in such circumstances, to say anything on this subject; but, as it can do no harm to offer an opinion, and may be useful in directing the attention of subsequent observers to the point, it may not be improper here to add one remark.

When all the circumstances of the species of lesion produced in this experiment are considered, it seems reasonable to infer that the effect of the division of the nerve is to suspend, or interrupt, or destroy the power of the minute capillaries in transmitting the blood through them. The larger and more visible arteries are distended; the parts become red and hot; the blood manifestly does not move from the large vessels into the small capillaries; this can be caused only by the circumstance that the capillaries are not transmitting the blood conveyed into them to that division of the capillaries which communicates with the veins. The blood accordingly, being stopped in its progress, is accumulated in the large vessels; and in this manner the increased heat of the parts is produced.

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[1] Physiological Researches. By Sir Benjamin C. Brodie, Baronet, D.C.L., F.R.S., &c. Collected and Republished from the Philosophical Transactions. London, 1851. 8vo, pp. 146. P. 31; and Note A., p. 105.

This explanation also accords with the temporary effects of the Galvanic Stimulus. We know not whether that stimulus acts upon the nerve. Probably it does. But the effect of this stimulus is manifestly to restore for the time to the capillary vessels a certain degree of their power of transmitting blood. The blood is moved through them; and consequently they receive blood from the arteries immediately larger, which open into their canals. These arteries hence are reduced to their normal dimensions. The parts lose their red colour and congested aspect; and at the same time the temperature of these parts falls to its natural rate. This return to the normal condition of the parts is only temporary, and lasts no longer than the action of the Galvanic Stimulus, which in this instance appears to perform the part performed in the healthy state by the Nervous filaments.

One word as to another point should not be omitted. The experimentalists speak of increased action of the vessels and acceleration of the circulation. This is an erroneous mode of expression. The action is diminished, impaired, almost annihilated in the capillaries. The movement of the blood is suspended, retarded, interrupted. It is only in the natural state that the action of the vessels is greatest, and that the circulation of the blood is most rapid. The first effects of disease are to impair and diminish action and power, and to retard the motion of the blood within the capillaries. Increased heat is associated with diminished power, impaired action, and retarded and partly-obstructed and ceasing movement of the blood.

If any useful inference is to be deduced from the experiment of dividing the great sympathetic, or destroying the part of the spinal marrow from which its nerves, shall we say filaments, proceed, that inference is, that the sympathetic nerve regulates and maintains the action of the capillary blood-vessels; and that, when this influence is destroyed, when its connection with its source or centre is cut off, the capillaries lose at least part, perhaps the whole, of their transmitting power, and the circulation of the blood through them is suspended and interrupted.

Part of the same effect is, that the processes of nutrition and secretion, in whatever these may consist, are disturbed, perverted, and suspended; and this disturbance and suspension is, with other circumstances, one cause of the greatly increased heat of the parts. In the healthy state, the existence of these processes tends to keep the temperature at its proper normal rate; but when these processes are disturbed with the disturbance in circulation, the increase of heat in the parts follows as an associated effect.

Before closing these remarks, it may be well to mention, that instances of this form of morbid heat are not uncommon in the natural progress of disease. It sometimes happens that aged or middle-aged persons are attacked by symptoms which at first sight seem not to denote very serious disease. The pulse is not quickened more than eight or ten beats. Slight shiverings are felt, but so trifling that they are often overlooked. The patient sleeps a little restlessly, but still he sleeps, during the night. During the day he is so little unwell, that he insists on being up, and probably moves about a little. There is no decided headache, no local uneasiness or functional disorder. The only thing that is distinctly wrong is incontinence of urine, which comes away involuntarily in small quantities from time to time, At length the patient is no longer able comfortably to keep out of bed, or be much up. The skin is rather dry, though neither hot nor cold. Perhaps it is in certain regions a little colder than it ought to be, as in the feet or the legs, and the hands. Treatment of various kinds is directed. The skin becomes after an interval, varying in different instances, warm, it may be said comfortably warm, and some moisture appears. It is sometimes supposed that this is to be the solution of the disorder. It is so; but not the favourable solution. The patient sleeps, and at length it is observed that he is insensible. The cutaneous moisture continues; but the surface becomes cool, and at length cold; and in no long time death follows.

This is not a very uncommon mode of the termination of life. It is entitled to mention in this place, because it consists in a peculiar lesion of the Brain, of the nature of slow congestion, affecting both the membranes and the substance of the organ. This lesion is, indeed, diffused over the whole bloodvessels of the organ. This seems to cause imperfect secretion of urine, and at the same time paralysis of the sphincter. The urine is not entirely suppressed; but urea circulates in the blood, and is even discharged by the skin. The energy of the brain is impaired, partly by an unnatural state of the vessels, which are usually rigid and slightly ossified, partly by the poisonous action of secretions which ought to be eliminated;—urea, biliary matter, carbonic acid. and perhaps other substances.

ART. III.—*Observations on the Structure and Development of Bone.* By JOHN TOMES, F.R.S., Surgeon-Dentist to the Middlesex Hospital, and CAMPBELL DE MORGAN, Surgeon to the Middlesex Hospital. (Philosophical Transactions for 1853, p. 109.)

[It is scarcely necessary to remind the readers of this Journal, that Mr Tomes is well known for his Microscopical Researches on the Structure of the Teeth, and that he has generally correct representations of the Bone and the Ivory of the Teeth. To the services of Mr Tomes in this department we had occasion to direct attention in the seventy-second volume, October 1, 1849.[1] Since the publication of his Lectures on the Anatomy and Physiology of the Teeth, Mr Tomes has, in conjunction with Mr Campbell de Morgan, been engaged in investigating the Structure and Development of Bone; and the result of these labours is an elaborate paper in the Philosophical Transactions. Of this paper it was our wish to present such an abstract as the subject and the manner in which it is treated deserves. But it appears to us that the most eligible method is to give the paper entire, without abridgment or alteration.]

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If the section be taken from near the end, instead of from the middle, of the shaft of a long bone, the Haversian canals will appear much larger, and in the place of one great central opening, we shall see numerous lesser ones, corresponding to the cancellous part of the bone. From whatever bone, or part of a bone, the section be taken, the arrangement of laminæ and lacunæ is substantially the same; the differences

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Dr Waller had made upon the question of augmented heat and increased vascular action additional researches. The object of Dr Waller was to shew that it is possible to regulate at will the phenomena which follow the division of the Great Sympathetic Nerve, so as to diminish or to increase, within certain limits, the activity of the circulation of the blood and the temperature of the neighbouring parts.

When we divide or inclose in a ligature the cervical portion of the Sympathetic Nerve, or subject it to the repeated stimulation of galvanism, the opening of the pupil is contracted, the *membrana nictitans* advances over the surface of the eyeball, and the vessels of the eye become more red. The temperature of these parts is increased to such a degree as to be easily estimated by the hand and by the thermometer. These phenomena continue in the same state for a very considerable time, ordinarily for several months. If we apply galvanism to the upper end of the nerve, the pupil is dilated to its greatest extent. The *membrana nictitans* is withdrawn into the angle of the eye; and at the same time if we examine the vessels of the conjunctiva and those of the ear, we perceive that they are empty, and that the skin and the mucous membrane return to their normal condition.

If then we withdraw the galvanic poles, the contraction of the pupil and the injection of the vessels soon reappear. Upon removing and approximating these poles alternately to the nerve, we make the ocular, vascular, and auricular phenomena appear and disappear alternately as we choose. While the injected vessels empty themselves of blood, the temperature of the skin falls; and Dr Waller was able in this manner to cause the thermometer to fall from one degree to one degree and a half of the centigrade scale.

The ocular and the auricular phenomena are closely connected together.

The action of the nerve is evinced almost exclusively upon the arteries and the capillary vessels. These vessels are contracted and at length obliterated, while the veins remain black and distended with blood. In a large rabbit, with pendulous ears, a variety common in England, towards the base of the ear, though the diameter of the artery was more than one line, the internal cavity became empty and completely disappeared under the action of galvanism. When the galvanic current ceased, the undulating blood appeared in the vessel.

The vascular turgescence produced by the action of topical

irritant substances, as very hot water, mustard, ammonia, disappears almost completely under the influence of galvanism. After division of the Great Sympathetic, the capillary hæmorrhage of the operated side is greatly more copious than that on the other side; this hæmorrhage diminishes or entirely ceases. If we galvanize the divided nerve, all these differences in animal temperature are especially remarkable in the cold season; for a temperature of fourteen degrees was required to establish the equilibrium.

In the month of February, when the temperature was below zero cent., the ear of the operated side presented a temperature of ten degrees above the unoperated side. This great inequality disappeared gradually, when the animal was placed in a medium with a higher temperature. At this time Dr Waller found in dogs the temperature of the ear of the operated side ten degrees cent. above that of the side not operated on, the latter being twenty-five degrees, = 77° F., while the former was thirty-five degrees, = 95° F. After the animal had remained half an hour in the chamber in which the operation was performed, where the temperature was at fourteen degrees cent., = 58° F., the unoperated side indicated a temperature of twenty-nine degrees, = 85°, while the side on which the operation was performed remained unchanged. After one hour the unoperated side was at thirty-four degrees, the operated side at thirty-five degrees cent.; and at these points the temperature of both sides remained stationary.

The inclosure of the artery in a ligature, or the inclosure of the jugular vein, appears not to affect the temperature of the ear of the operated side; but upon applying galvanism to the lower end of the pneumogastric nerve, divided so as to weaken greatly the action of the heart, a depression of one degree of the centigrade scale takes place.

The arterial contraction is equally produced by irritating by galvanism the Great Sympathetic Nerve in every part of the cervical region. The centre of this nervous action is found in the Spinal Marrow. The maximum effect is produced by galvanizing the spinal marrow at the level of the articulation of the second and third dorsal vertebræ.

After division of the Cervical Sympathetic Nerve, even after applying galvanism to it several times, the part of the nerve which is above, is disorganized. Further, at the end of four or five days, galvanism applied to this chord from the point at which it has been divided, or galvanically stimulated, to the first cervical ganglion, produces no effect upon the vessels of the ear. But if at this time we apply galvanism to the first cervical ganglion, we produce at once constriction

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The presence of persistent nuclei in the lacunæ has been questioned by recent authors; some state that they may be detected at times, while others regard their presence as very problematical. Mr Goodsir[1] describes the lacunæ as containing a little mass of nucleated cells.

M. Kölliker,[2] after citing Schwann, Krause, Kohlrausch, Fleischmann, Günther, and Donders, all of whom believe in the persistence of the nuclei in the lacunæ, goes on to say that he has paid great attention to the subject. In the bones of an individual eighteen years old he found them after the sections had been treated with acid. The authors have had no difficulty in finding the nuclei in recent bone without the aid of chemical treatment. If a small fragment be taken from the spongy portion of a fresh bone and freed from adherent fat, the nuclei may be seen as small rounded bodies attached to the walls of the lacunæ; their appearance is shewn in Plate IX. figs. 27 and 29, *Tr.* In dry sections of bone the nuclei are less readily recognised, although we may find them here and there. The authors have sections of a fossil bone from the Wealden (supposed to be Pterodactyle) in which all the lacunæ have well-defined nuclei (*Plate*, fig. 5).

4. *Haversian Systems.*—Having described the several parts entering into the formation of Haversian systems, it now remains to say a few words on the relations of these to each other, and to the other parts of bone which are not included under that term. The union of the Haversian and other systems of laminæ is seen, in a transverse section of a long bone, to be effected by the interposition of a layer of transparent tissue. In a recently-formed system the canaliculi do not appear to pass through this layer, but in older ones they are seen to pass across from the external lacunæ and to anastomose with those from the interstitial or circumferential laminæ, or with those of adjoining Haversian systems (Plate VII. fig. 10, *Tr.*) After these intercommunications have been fully established, the connecting layer loses something of its original transparency, but not the whole; for in the bones of aged subjects it may still be distinguished.

In Dr Todd and Mr Bowman's "Physiological Anatomy," it is stated, on the authority of Mr Tomes, that the canaliculi of adjoining systems anastomose but scantily. A more extended examination into the subject has shewn this to be true of the more recently developed systems, but not those of greater age. In addition to the connection of systems of laminæ by the juxtaposition of their external members, instances not unfrequently occur where several Haversian systems, linearly arranged, are encircled by a series of laminæ common to the whole. This is shewn in Plate VI. fig. 5, *Tr.* But although we may find groups of systems thus bound together by inclosing laminæ, in addition to the general or partial investment by circumferential laminæ, such binding together is clearly not necessary to ensure the strength of the bone, as we see many bones altogether destitute of circumferential laminæ, and very few with these laminæ complete; while the encircling of several Haversian systems with laminæ common to the whole, is the exception rather than the rule.

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[1] Anatomical and Pathological Observations, by John Goodsir and Henry D. S. Goodsir, 1845.

[2] Kölliker, *oper. cit.*, p. 296.

In the progress of their investigations the authors have failed to discover any instances in which pre-existing laminæ, partially absorbed, have become continuous with newly-developed ones, end to end; indeed, there seems good reason for supposing that such seldom if ever occurs, otherwise the Haversian systems would not be so strongly marked, neither should we have the festooned outline to the systems lying against the cut ends of the laminæ, in the manner shewn in *Plate*, fig. 1, and Plate VII. fig. 10, *Tr.*

In examining Haversian systems divided transversely, one will be occasionally found in which the canal has been completely obliterated by the development of a lacunal cell in its centre. This condition is illustrated in fig. 1 *c.*

5. *Ossified Cartilage of Joints.*—Dr Sharpey has shewn that beneath the articular cartilage lies a layer of bone produced by the gradual ossification of this tissue.[1] Professor Kölliker[2] has observed this form of bone to be present in all the articulations, excepting that of the jaw and the hyoid bone, in which situations he has failed to detect it. The authors have numerous sections from the articular process of the lower jaw, in all of which it is present, and although in smaller amount than is usually found in corresponding parts of other bones, still it is sufficiently distinct. The peculiarity of the articular bone consists in the maintenance of the same arrangement of parts as existed in the cartilage prior to its conver-

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[1] Dr Quain's Anatomy, Fifth Edition. By Mr Quain and Dr Sharpey, Part II., page 158. "In the slow growth of bone which encroaches on the attached surface of articular cartilage, the ossification would almost seem to be produced merely by the impregnation of the cartilaginous matrix with earthy matter (corresponding with the first step of the ordinary process); and in this case, the cells and clusters of cells being surrounded by calcified matrix, may remain as little vacuities or lacunæ in the bone; but this, as well as the formation of lacunæ in the crusta petrosa of the teeth, and the production of adventitious bony deposits in different textures, requires further investigation."

[2] Kölliker, page 318, *oper. cit.*

sion into bone. The cells for the most part are arranged in groups or in lines parallel with the long axis of the bone, each cell presenting a roundish mass surrounded by granules, and rendered rather indefinite in outline. Not uncommonly several cells become connected end to end, thus producing an elongated form. It is when in this condition that the cells and basement tissue may be restored to the appearance of cartilage by the use of hydrochloric acid. Occasionally, however, a cell becomes converted into a well-developed lacuna, and near the ordinary bone the cells may be seen in more or less advanced stages towards their conversion (Plate VII. fig. 17, *Tr.*)

In the ossified cartilage, when the tissue attains a considerable thickness, broad and ill-defined lines or bands of opacity may generally be found running parallel with the articular surface, and giving a stratified character to the part in which they are situated, as shewn in fig. 17 *b*. When seen by transmitted light, these lines appear of a deep rich brown colour, and as if possessed of a much higher degree of granularity than the surrounding parts. The cells of the tissue are not uncommonly surrounded by a similar granular condition of the matrix. This state of granularity is most conspicuous near the bone, and less so near the cartilage, where the tissue becomes clear and transparent. The ossified cartilage is generally separated by a well-marked line from the subjacent bone. Exceptions to this, however, are not wanting, where the two pass, the one into the other, by insensible gradations. The bone of the line of junction is usually advanced into the ossified cartilage by rounded projections of variable size; some of which reach nearly to the surface, while others are situated at some little depth. From this arrangement the articular bone is necessarily irregular in quantity. In one part it will dip to a considerable depth into the bone, while at another, and perhaps contiguous part, it will form but a thin layer. The external articular surface is but slightly affected by the irregularities of the surface attached to the bone; on the contrary, it presents an even surface, on which the articular cartilage is placed in a uniform layer.

The absence of Haversian canals, and the unfrequency of lacunæ with canaliculi in the articular bone, renders this tissue much less porous than ordinary bone, a condition which probably contributes to its strength, and to the strength of the part in which it occurs.

In the presence of this peculiar form of tissue at the articular extremities of bones we may perceive a striking instance of design.

It would appear that the articular cartilage must rest upon a firm and uniform surface, otherwise it would, under pressure, yield unequally, and the joint would probably be but imperfect in its action. Circumferential laminæ are developed only on free surfaces, and do not present there a a very level outer surface. A surface formed by Haversian systems would be irregularly fluted or nodulated, like that beneath the calcified cartilage. Neither of those forms of development, then, would effect a condition of surface similar to that attained by the ossified cartilage. A careful consideration of the foregoing facts will, we think, lead to the conviction, that the presence of ossified cartilage as a basis for articular cartilage must not be regarded simply as the result of a slowly-advancing ossification of a tissue prone to ossify; neither must it be regarded as an imperfectly-developed tissue in which the formative process was arrested before the part had been perfected;[1] but it must be regarded as a constant element in the osseous system, having its special use, and necessary to the perfect organization of the skeleton in the higher forms of Vertebrata.

6. *Ossified Lacunal Cells.*—In addition to the parts already described, the authors have found a condition of bone which has hitherto escaped observation, or if seen, has not been recognised as a form of osseous tissue. The bones of aged people not unfrequently become extremely light and spongy, readily break, and from the diminished amount of compact tissue, may in the case of the flat bones, such as those of the pelvis, be indented by firm pressure with the finger. Such bones, after maceration, contain within the spaces inclosed by the cancelli, a white powder, which readily falls out if the bone be broken. If a little of this powder be mounted in Canada balsam, and placed in the field of the microscope, the observer will see, that it is mainly composed of large nucleated cells, some of which are detached, others united into masses.

The isolated cells have a spherical or oval form, and appear to consist of a granular nucleus surrounded by a very thick cell-wall, external to which we commonly find an aggregation of granular matter, indefinite in amount and presenting a ragged outline (Plate VII. fig. 15, *Tr.*) The cells which are united into masses are connected by this granular matter, or ossified blastema, as it might perhaps be called. The cells themselves are similar to the loose ones, excepting that in some cases their character is rendered more apparent by the nucleus assuming the form of a lacuna. If a section of the bone be examined, similar cells will be found adherent to the walls of the Haversian canals, with the canaliculi of contiguous lacunæ advancing into them, while the nuclei of the adherent cells are assuming the form of lacunæ and throwing out canaliculi.

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[1] Kölliker, *oper. cit.*, page 318, speaks of this tissue as imperfectly-formed bone-substance.

Again, in the substance of the bone, the outline of the formative cells, similar in size and shape to the loose ones, may be in places recognised, having lacunæ and canaliculi as their centres. The recognition of these ossified cells in their isolated state, will be followed by the observation of similar cells occupying the Haversian canals and cancellar spaces in the majority of microscopic preparations of adult bone, (Plate VI. fig. 5 *a*, *Tr.*); and the same may sometimes be seen on the outer surface of bone (Plate VII. fig. 19, *Tr.*) The appearance of the cells is, however, liable to be modified by the various circumstances which operate upon growing bone, such as the presence of a tendon, or of a large pulsating vessel, or in fact by pressure from any cause. Thus if situated in the immediate neighbourhood of a tendon, they assume an elongated form; while on the other hand they may be flattened by pressure.

It has been thought desirable to describe the ossified lacunal cells at this place, on account of their frequent occurrence in adult bone (Plate VI. fig 5 *a*, and Plate VII. figs. 16 and 17, *Tr.*)

7. *Bone Tissue.*—Hitherto we have confined our description to the forms in which the bone substance is arranged, as into laminæ, Haversian systems, &c. We have now to consider the ultimate structure of the tissue itself. But little has been said upon this point until within the last ten years. In the "Physiological Anatomy" of Dr Todd and Mr Bowman,[1] it is stated that bone is made up of an aggregation of granules in a scarcely distinguishable matrix. Latterly, other views have been proposed. Dr Sharpey has advanced the hypothesis, that although the bone developed in cartilage is at one period granular,[2] a reticulate fibrous arrangement of the ele-

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[1] Dr Todd and Mr Bowman, Physiological Anatomy, page 108.

[2] Mr Quain's Anatomy, edited by Mr Quain and Dr Sharpey, page 157. "The primary osseous matter forming the original thin walls of the areolæ is, as Mr Tomes observes, decidedly granular, and has a dark appearance; the subsequent or secondary deposit, on the other hand, is quite transparent, and of a uniform and homogeneous aspect, without obvious granules. This begins to cover the granular bone a short distance (about the $\frac{1}{30}$th of an inch) below the surface of ossification, and, as already stated, increases further down. The lacunæ first appear in this deposit; there are none in the primary granular bone. In what further regards the nature and formation of the secondary de-

ments of the tissue takes it place, and must be regarded as the general condition of bone.[1] Dr Sharpey, when he formed this opinion, had directed his attention principally to thin fragments torn from the surface of bone macerated in hydrochloric acid. The authors have seen the appearance both in decalcified bone, and in sections which have not been chemically treated ; but they believe the fibrous appearance to be an optical effect produced by the canaliculi and granules when the light is unequally distributed, or the object-glass in bad adjustment. It will be shewn in the subsequent details, that the appearance can be produced and dispelled at pleasure by alterations in the direction of the transmitted light, If thin sections of bone are seen by transmitted light, (passing through in the axis of the microscope), the tissue will appear either granular or structureless, and in the laminæ alternations of granular and structureless parts will be seen. But if the light be allowed to pass in one direction only, and with considerable obliquity through the specimen, then the appearance of extremely minute fibres will present itself ; and this not only in adult bone, but in the isolated cells of old and in developing young bone. They are most strongly marked over the lacunæ ; to which, and to the canaliculi, a confused and broken outline is given. The fibres appear as though

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posit, my own observations lead me to differ considerably from the views of Mr Tomes. He supposes that it is formed of cells which become impregnated with earthy matter,—the cartilage cells in the first instance, and afterwards the cells newly formed in the blastema. Now although certain appearances render it not improbable that there may be a layer of flattened and calcified cells next to the surface of the granular bone, I am nevertheless disposed to think that the subsequent and chief part of the deposit results from the calcification of successive layers of fibres generated in the blastema, and possibly derived from the granular cells, some cells being perhaps also involved along with the fibres, as in the ossification of the flat bones of the cranium : in short, it appears to me that the deposit in question is formed after the manner of intramembranous ossification already described (page 150.) I infer that such is the process from the structure of the layers, for they are made up of fine reticulated fibres like the lamellæ of perfect bone shewn at page 143."

[1] On this point Kölliker, *oper. cit.* p. 289, observes, " Die Grundsubstanz der Knochen besteht nach Sharpey und Hassall aus einem Netzwerk feiner Fasern mit rhombischen Maschen, von welchen Fasern ich weder an frischen, noch an mit Reagentien behandelten Knochen eine Spur habe entdecken können. Was Arnold neulich Primitiv-fasern der Knochen nennt, sind, wie seine Abbildungen lehren, nur die Granula der Grundsubstanz, an denen er eine lineare Aneinanderreihung zu erkennen glaubt, und seine Querstreifen auf Flächenschnitten, und radiären Linien auf Querschliffen, wenigstens die letzteren sicher, nichts als die Auslaüfer der Knochenhhölen. Sollten durch irgend ein Reagens in dem Knochengewebe deutliche Fibrillen nachgewiesen werden können, was mir aber noch nicht gelungen ist, so könnten dieselben wohl keine anderen sein, als diejenigen, welche in ossificirenden Knorpel das streifige Ansehen bewirken und die von Sharpey und mir bei der Knochenbildung aus dem Periost beschriebenen."

arranged in series of short and broken lengths, parallel and without plaiting. If the specimen be a little out of focus, there will be an appearance of reticulated fibres marked by nebulous outlines. On the other hand, if we have light passing from all sides obliquely through the preparation,—that is, if the object be illuminated by a hollow cone of light, in the manner obtainable by Mr Gillett's arrangement of achromatic condenser, or by rendering opaque the centre of the lower lens of the ordinary achromatic condenser,—the fibres disappear, and we see in their place a granular tissue, with a tendency here and there to a linear arrangement of the granules, and the lacunæ and canaliculi clear and distinct, when well in focus, but with a fibre-like nebulosity when slightly out of focus, especially in those parts where the latter run obliquely through the specimen. This is more especially the case, where the canaliculi are filled with fluid, or where they have become solid,—a condition very common in the bones of old subjects. Indeed, in many specimens where the bone is not highly developed, a fibrous appearance at once strikes the eye, and it is only on careful examination of the surface of the section that the illusion is dispelled by the orifices of the canaliculi appearing at the termination of the apparent fibres. Amongst a great many examples, the authors have not seen one instance in which the fibrous appearance could not be traced to canaliculi, this effect being often increased by the presence of granules. If parallel dentinal tubes be viewed by a side light, in the manner we have described, each tube will throw two or three strong nebulous lines, which might be mistaken for fibres; and if tubes cut transversely be inspected in a similar manner, each will appear to have several concentric lines extending half round it on the side opposite to that from which the light proceeds.

That the presence of granules imbedded in a less refractive tissue may, when viewed by a side light, give an appearance of fibres, is shewn on an examination of pus-globules under similar circumstances, when each globule will seem to have a striated surface; and that slightly-rounded prominences will produce a like effect is shewn in the case of some species of *Navicula*. The surface of these latter objects is marked by rows of slight prominences, which, when the light is oblique, and passes through the specimen from one point only, appear as lines or fibres, the direction of which will change with alteration in the direction of the light; that is to say, shadows will extend from one eminence to another in the opposite direction to that from which the light enters. If we examine the projecting spiculæ of bone forming in temporary cartilage under corresponding conditions as regards light, a similar fibrous appearance will be developed.

Under these circumstances the authors are forced to the conclusion, that bone substance, instead of being made up of minute reticulate fibres, is composed of granules or granular cells, imbedded in a more or less clear homogeneous or subgranular matrix. This subject will receive further elucidation under the head of Development. These views are supported by the appearances presented, not only in developing bone of young subjects, but also in the bone of adults.

Thus, as regards the basement homogeneous tissue, it will be found that where lamination is highly developed, the laminæ have a transparent, and structureless, and a more opaque and granular part, to which the former appears to be the matrix. The peripheral lamina of the Haversian systems is generally clear and free from granularity, and the internal lamina sometimes presents a similar structureless appearance. The matter which fills up the Haversian systems in the full-grown antlers of the Cervidæ affords another and a very striking example of transparent structureless osseous tissue, which in this instance is the more distinct from the absence of canaliculi in its substance. Then, again, we have another instance in the clear tissue, which is sometimes found between the superficial Haversian systems of ordinary bone. It has already been described as a non-laminated element found on the *surface* of certain bones. In the instances already cited, and no doubt in many others, which may be found in the skeletons of the lower vertebrata, we have bone tissue without obvious granularity, and without obvious structure; and although it forms but a small part of the general mass, yet, from its constant presence at all ages and in all subjects, it must be regarded as an integral and normal part of mammalian bone. The granular condition of bone tissue is tolerably obvious in all preparations, though it is much more strongly marked in some specimens than in others. The amount of the component granules varies in different parts of the same specimen, and in specimens taken from different parts of the skeleton. Thus in one situation we may see laminæ with a highly granular part gradually merging into a transparent tissue, while in another the laminæ may be granular throughout. Again, in young bone developed in cartilage, the part between the cells becomes highly granular; fragments of which may be found in certain adult bones, as in the petrous portion of the temporal bone (Plate VII., fig 16 *b*, *Tr.*) Bone near the articular surface frequently presents a well-marked granularity. Whether the granular and the transparent parts of bone contain different relative amounts of phosphate of lime, is a question yet to be decided.

8. *Development of Bone.*—Temporary cartilage, when it first appears in the embryo, consists of an aggregation of closely-packed nucleated cells, which in the process of growth become separated by the development of a tissue external to them, usually designated the hyaline tissue of cartilage. At this stage of growth we have a granular cell occupying a cavity in the hyaline tissue; at a previous period the granular cell was inclosed by an outer cell-wall, to which it formed the nucleus, but now the cell-wall has merged into the hyaline tissue, from which it can no longer be distinguished, while the granular cell contains itself one or more nuclei. In making sections of cartilage many of these granular cells escape from their cavities in the hyaline tissue, and may be seen detached and floating about in the field of the microscope. Some little confusion of terms has arisen in connection with this part of the subject. The granular cells are not unfrequently described under the name of cartilage-nuclei containing nucleoli, while in fact they are cells capable of separation from the matrix, and are at one period the only recognisable cartilage cells. They occur singly or in groups; more frequently, however, two or three are extended in a line. It is proposed in the present communication to call these bodies *granular cartilage cells*, as distinguished from a form which the cells assume previous to ossification.

Cartilage previous to its conversion into bone undergoes a rapid growth, which takes place principally in the direction of the long axis of the future bone. Each granular cell becomes divided into two by segmentation transverse to the line of ossific advance. These are again divided, and the process repeated from time to time, till in the place of a single granular cell, we have a long line of cells extending from the unchanged cartilage to the point where ossification has taken place. Contemporaneously with this development of lines of cells, other changes are going on in the individual cells composing them. If we examine those situate near the advancing bone, it will be observed that the granular cells have enlarged, have become separated from each other by wide intervals, and that each has become invested with a thick pellucid cell-wall (Plate VIII., figs. 19 and 20, *Tr.*) The increase in the size of the cells has occurred at the expense of the hyaline tissue, which, at those points where the rounded cells approach each other, is reduced to a thin film, although in the intervals left by the packing of the uncompressed cells, it exists to a considerable amount, Here, then, we have a matrix containing cartilage cells composed of an outer pellucid coat, within which is a granular cell containing central nuclei, a cell consisting of three distinct parts. The nucleus existed previous to the segmentation of the granular cell, but the outer wall is the product of a subsequent development. It will be observed that growth is effected by two modes; first, by the increase of the number of the cells, and secondly, by the increase of their size individually.

While these changes for lengthening the shaft of a long bone are going on, cells are being added by a somewhat similar process of division to the surface, so as to increase the diameter of the cartilage. For the development of the epiphysis the cells become multiplied on a similar principle, but instead of occurring in lines they are accumulated in oval or rounded groups.[1]

From this general view of the subject we will now proceed to consider in detail those conditions of cartilage which precede and prepare for its conversion into bone. These phenomena may be conveniently examined in the cartilage which connects the diaphysis and epiphysis of long bones, and we may take those of a lamb or calf, as they present the same conditions as are found in the human subject, and have the advantage over those of other domestic animals of being readily obtainable at all times, without the necessity of destroying the animal for the sole purpose of physiological investigation.

At that point in the cartilage where the linear arrangement of cells commences, several of these bodies lie side by side in the same column, but by following the line downwards towards the advancing bone, it will be seen that the column usually divides, forming two or three distinct lines, in each of which the cells are arranged in single file. It does not appear that the number of cells entering into the formation of a series are at all limited; hence the lines or columns vary in length. Neither does it appear that the consecutive lines are placed, as a rule, end to end, so as to form one long continuous straight line by the junction of several shorter ones. On the contrary, each series is complete in itself, and is generally placed a little to the one or other side of those above and below it. We have, in fact, interrupted lines of cells, each line the offspring of a single cell, the outer walls of which have merged into the intercellular or hyaline tissue.

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[1] Previous to the ossification of the epiphysis, the cartilage at the heads of the bone is increased in its bulk from additions to the surface, by a process somewhat similar to that through which the growth of bone is effected. This process will be described when the latter subject is considered.

When the destined number of cells forming a linear series has been developed, each cell becomes itself a centre of growth. The granular cell enlarges, together with its nucleus, and becomes invested with a cell-wall. In examining a line of these bodies extending from the forming bone of the diaphysis, we shall see them in various degrees of forwardness. Thus, if attention be directed to the end of the line furthest from the bone, the cells will be found small in size, granular, and with a perceptible nucleus, but without an outer wall, distinguishable from the hyaline substance, which is abundant between the contiguous lines, but small in quantity between the cells composing the lines. But if the other end of the line be examined, very different conditions will be observed. The granular cells will be seen to have become rounded in form, to have increased to three times their original bulk, and to possess well-marked circular nuclei; in addition to which each granular cell will have acquired a thick pellucid outer wall, while the hyaline tissue between contiguous lines of cells will have dwindled down to a thin film, excepting in those parts where spaces are necessarily left in the approximation of spherical bodies (Plate VI., figs. 19, 20, *Tr.*) The abundanee of the hyaline tissue in the earlier condition of cartilage affords space for the development of the cells in the breadth of the bone; but in the direction of the length of the bone we find but little of that tissue, yet each component cell of the almost innumerable lines of cells that exist, even in a bone of small size, is itself a centre of growth. At the osseous end of a line a single cell occupies more space than four or five at the opposite extremity; and, as in each situation the cells are imbedded in hyaline tissue, it is evident that there is a concurrent growth of the latter throughout each line of cells, and also of the cells and hyaline tissue in the breadth of the cartilage. But for this provision of nature, cells would grow at the cost of those in their neighbourhood; a condition which obtains wherever space for a secondary development of bone is required, a description of which will follow in the subsequent pages of this paper. Then, again, had we concurrent growth in each cell of a series without a similar growth in all the other series throughout the breadth of the bone, the growing series would encroach upon the cartilage in which the linear arrangement had not commenced, and ultimately unite by osseous tissue the epiphysis and diaphysis before the bone had acquired its normal length. And supposing concurrent growth to take place over the one-half of the breadth of a bone and not in the other, the bone would become curved and the limb distorted.

We have traced in temporary cartilage those changes which occur preparatory to its conversion into bone; it now remains for the authors to describe the results of their researches on the mode in which that conversion is effected, and concerning which there hitherto has been a great diversity of opinion.

Preceding ossific deposit, the intercolumnar tissue, which may not only be seen between the columns of cells, but also passing more or less perfectly between the individual cells, becomes in some cases slightly fibrous in appearance and of a light brown colour; this condition speedily gives way to a highly granular state; in fact it has become bone, and incloses, in osseous cavities or crypts, the cartilage cells. For reasons which will hereafter be sufficiently obvious, we shall in future call these lacunal cells. If thin sections be made with a very sharp knife through ossifying cartilage, and placed in water or albumen under the quarter or eighth object-glass, we shall occasionally find lacunal cells which have escaped from their osseous crypts, floating loose in the fluid, and offering a favourable opportunity for the examination of their characters. It may then be seen that the thick pellucid outer investment has become granular, and that the inclosed granular cell has lost its regular outline, and become angular or ragged, while the nucleus has become obscured by the changes in the more external parts of the cell, an early stage of which is seen in Plate VIII. fig. 19 and 22 *a*, *Tr*. Such are the appearances presented in some of the detached lacunal cells, but they may be seen in both earlier and later stages of development than that described. Thus instances may be found where the outer coat has only a few granules on the surface, in which case the granular cell may be seen more distinctly, and minute elongated processes traced, extending from its surface (Plate VIII. figs. 19 and 20, *Tr*.) In a favourable section we sometimes find such a cell with its outer coat torn, exposing to full view the granular cell, with numerous and well-marked processes extending from it. Dr Sharpey kindly placed at the disposal of the authors the rickety bone of a child who died during the active condition of the disease. In this specimen granular cells with radiating processes can with great readiness be detached by tearing a thin section; for in this case cartilage assumed the arrangement of bone without the impregnation of earthy ingredients. (Plate VIII. fig. 23).[1]

The third condition, in which we may find isolated lacunal cells detached from the osseous crypts of the intercellular tissue, differs from those already described in their greater solidity. The whole mass has become highly granular, and the granular cell with its processes has united to the outer coat, so that the two can no longer be separated the one from the other, and are no longer to be recognised as distinct parts. If accidentally broken across, we see that they have a hollow centre, in fact, a lacuna; but when loose and entire, they appear as rounded dense masses projecting from the surface, of which we may not unfrequently detect a few short needle-like processes. Lacunal cells of the latter kind are not, however, very easily detached, for when they have advanced thus far in development, their union with ossified intercolumnar tissue is too strong to admit of ready separation; indeed, after this period their separation is impossible. It has been stated that the granular cells of cartilage previous to the formation of lacunal cells send out processes. There are, however, a certain number of granular cells that do not appear to undergo this change, but preserve their state of granulation and their external figure, and shew the round nucleus even after they are surrounded by and imbedded in ossified tissue. Indeed, we may find perfectly ossified crypts of intercolumnar tissue at some little depth within the forming bone, containing several of these granular cells in an unaltered condition, and this without the cavities having been enlarged by absorption of their walls (Plate VIII. fig. 19 *d*, *Tr.*) It is important to bear this point in mind, as it will subsequently be shewn that similar granular cells lie loose in the large cavities formed by absorption, and that they are concerned in the formation of bone when that tissue is not preceded by ordinary cartilage.

If a section of developing bone, including a little of the cartilage, be dried, and mounted in Canada balsam, we shall be able to recognise, first the cartilage with the cells greatly contracted, then the ossified intercolumnar tissue full of little crypts, from which, in the process of drying, the lacunal cells have disappeared (Plate VIII. fig. 24 *a*, *Tr.*); and lastly, we shall see bone in which ossified lacunal cells run in lines corresponding to the preceding lines of cartilage cells (fig. 24 *b*). The latter appearance is, however, interrupted by the presence of numerous large spaces produced by the absorption of both the intercolumnar tissue and the lacunal cells (fig 24 *c*), the process for the removal of which commences concurrently with the impregnation of the lacunal cells with the earthy salt.

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[1] Kölliker, *oper. cit.*, at p. 360, describes accurately the formation of lacunæ from cartilage cells in a rickety bone, and, although he has not observed the changes with equal distinctness in normal bone, appears to have satisfied himself that in the ossification of cartilage the cells of the latter become converted into lacunæ. M. Kölliker does not however seem to have recognised the relation of the lacunal cells as such to the intercellular tissue, neither does he mention the fact that the granular cells with their long stellated processes can be separated from the investing cell-wall in the manner described in the text.

These conditions can be also very favourably observed in sections of the long bones of a fœtal lamb, taken previous to the commencement of ossification of the epiphysis, and examined in the liquor amnii; water will not answer as a substitute for this fluid, as the cartilage cells are speedily altered in character by its presence. And we may here observe that the examination of developing bone should be conducted on perfectly fresh subjects, and that care should be taken to keep the knives, glass, &c., which are used in preparing the sections, perfectly free from extraneous matter; otherwise the results will be unsatisfactory. Thus a small quantity of spirits of wine or of acid will produce changes of character in the specimen, and thereby lead the observer into an erroneous conception of the normal characters of the tissue.

Hitherto the authors of this communication have spoken principally of those appearances which are presented in sections made parallel with the advancing bone. Attention must now be directed to sections made across the bone, so as to cut transversely the lines of cartilage cells.

If, then, we take a thin section through the cartilage where ossification of the intercellular tissue is advancing, the following appearances will be seen. The intercellular tissue will present itself in the form of more or less perfect septa lying between and inclosing the lacunal cells, in such a manner as to produce a uniform pattern over the surface of the section (Plate VIII. fig. 20, *Tr.*) On close inspection, it will be seen that the intercellular tissue is granular, and that in each inclosure, it encircles one or two lacunal cells, which in the latter case lie in contact, with the contiguous sides flattened, while the peripheral surface of the two form together a tolerably perfect circle in close contact with the intercellular tissue.

The lacunal cells are beautifully shewn in the kind of section under consideration. Their circumference is becoming granular, and frequently may be seen as a perfect ring of granules in close contact with the intercellular tissue, while in the centre we see the granular cell with a well-defined nucleus, and with numerous rudimentary canaliculi, extending from the circumference of the inner cell towards the surface of the outer cell-wall. Where two cells lie in contact, the rudimentary canaliculi may sometimes be found running across from the one cell to the other. If after examining such sections as may be made with a knife, we take one through the bone immediately below the partially ossified cartilage, rendered thin by grinding, and mount it in Canada balsam, the various points described as existing in ossifying cartilage will be seen in that which has been converted into bone. Thus we shall see the intercellular tissue preserving its original form, and highly granular, and that while the outer walls of the lacunal cells have become calcified, the granular cells will have assumed the form of perfect lacunæ and canaliculi, the latter freely intercommunicating where the surface of the lacunal cells is in contact, but seldom extending into the ossified intercellular tissue. These conditions are shewn in Plate VII. fig. 25, *Tr*.

The ossified lacunal cells, and the intercellular tissue which have existed in the temporary cartilage, and have there served their part in forming the skeleton of the fœtus, are destined to have but a short existence, for we find that no sooner is the bone formed than large spaces are produced by absorption. Whole lines of ossified lacunal cells with the inclosing intercellular tissue disappear, to be replaced by more or less perfectly developed Haversian systems, which also last but for a time, when they by a similar process are removed, to give place to others, or to contribute to the formation of permanent cavities. It has been well stated by Dr Sharpey, that the long bones of the fœtus are not equal in size to the medullary cavities of the corresponding bones of the adult. Hence as growth takes place, gradually comes the necessity for the removal of parts, to make way for the development of others upon an enlarged scale.

It has been shewn that primary bone, if we except two or three of the cranial bones, is merely calcified cartilage which has previously assumed the structural arrangement of bone. It is proposed to restrict the term primary bone to tissue so formed, as it will be subsequently shewn, that the development of Haversian systems in the spaces formed in primary bone, is effected by processes similar to those which are concerned in the growth of bone generally, and in the extension of the flat cranial bones. Age and situation no doubt modify the conditions of growth, or development of secondary bone, as it may be called, but the processes in either case are alike.

Hence, in describing the manner, in which a long bone increases in diameter, we shall in fact be describing the process by which a flat cranial bone is extended. Before however going to this point the authors will state the results of their observations on the subject of the absorption of bone, on that interesting and important process by which the Haversian spaces are formed out of the sold tissue of bone.

During the present winter it became necessary to remove a portion of the femur which protruded from a stump six weeks after the removal of the limb. From the medullary cavity a granulating mass projected and covered the surface of the bone left by the saw; and as the bone was rapidly wasting from the inner or medullary surface, we had in this specimen a favourable opportunity of examining the tissue which lay in immediate contact with the surface of the wasting bone. On cutting through this piece of femur in its length with a very fine jeweller's saw; it was found, that a dense pale pink tissue lay in contact with the inner surface of the bone, which was hollowed with numerous minute cavities, into which the soft tissue accurately fitted, but from which it could be detached without tearing. The outer surface of the bone had been deprived of membrane many days before its removal from the limb.

The examination of the tissue thus closely applied to the fast-wasting bone, offered as favourable an opportunity for learning something of the means by which absorption is effected as we could reasonably expect to obtain; the more so since the outer surface having been for some time exposed and covered only by dried periosteum, the actions had been confined to the inner surface of the bone. A careful examination shewed that the surface of this tissue was composed of minutely granular nucleated cells, which lay in close and immediate contact with the bone, and increased in an exact ratio with its diminution. What the bone lost in bulk, the cells gained, the cellular mass presenting a perfect cast of the surface of the bone, suggesting to the mind that the soft was growing at the cost of the hard tissue, or at all events that the former was instrumental in the removal of the latter. The cellular mass was tolerably vascular, but the vessels did not reach the surface in contact with the bone; hence they could not be regarded as having any immediate action in the process of absorption. Section of the bone shewed that the medullary cavity had been greatly enlarged by absorption, and no doubt had sufficient time been allowed the femur at that part would have been reduced to a thin scale. A transverse section shewed that in many, though not in all instances, the Haversian canals had been enlarged and rendered irregular in shape, but it was evident that the process of removal had been less active in this situation than on the medullary surface of the bone.

If we examine the fangs of temporary teeth, when they are undergoing removal, similar states to those described as existing in the portion of femur will be found to obtain. A similar cellular mass will be seen to be closely applied to that surface of the tooth, which is in process of removal, and the surface itself will present the characteristic emargination observed in the bone. When we connect these conditions with the fact, that the nucleated cells which form the embryo have the power of appropriating the material which lies about them to the purpose of their own growth and their conversion into the various animal tissues, it is difficult to resist the belief, that the cells which lie in contact with wasting bone and dentine, take up those tissues and use all or part of their element for the purposes of their own increase or multiplication, or else form a medium through which they are passed into the circulation. But as the process of absorption with concurrent development of cells, is most active in primary bone, where but few vessels exist, the former hypothesis seems the more probable. An objection may be raised to the supposition, that the bone is absorbed by the cells, on the ground of the density of the former; but it must be borne in mind, that as the density is gradually imparted to the bone through the agency of the adjoining soft parts, there seems no good reason for disbelieving that they may also be instrumental in its removal.

9. *Growth of Bone.*—Under this head will be described the extension of flat bones, the increase in the diameter of cylindrical bones, and the development of Haversian systems.

If the advancing edge of a parietal bone be taken either from a human fœtus or a fœtal lamb, and the pericranium and dura mater be carefully removed from their respective surfaces, we shall find the growing bone still invested with soft tissue both on the outer and inner surface, which is prolonged from the free edge. When examined under a favourable light, this tissue will shew differences of character in different parts, varying with the distance from the bone at which the observations are made. Thus, if attention be directed to the part furthest removed from the bone, it will be seen that the membrane-like mass is composed of oval cells with slight prolongations from the extremities, which are frequently arranged in the form of bands of fibrous tissue (Plate VIII. fig. 26, *Tr.*) Dr Sharpey has observed that the membrane into which the bone extends is like fibrous tissue in an early stage of development,[1] and this observation is strictly true when confined to the part indicated, but the analogy ceases as we extend our examination towards the bone. Here in the place of cells with elongated processes, or cells arranged in fibre-like lines, we find cells aggregated into a mass, and so closely packed as to leave little room for intermediate tissue. The cells appear to have increased in size at the cost of the processes which existed at an earlier stage of development, and formed a bond of union between them. Everywhere about growing bone a careful examination will reveal cells attached to its surface, while the surface of the bone itself will present a series of similar bodies ossified. To these we propose to give the name of *osteal cells*, as distinguished from lacunal and other cells.

In microscopic characters the osteal cells closely resemble the granular cells of temporary cartilage, so closely indeed, that the latter when detached from the cartilage could not well be distinguished from them. They are for the most part spherical or oval in form, and lie on the surface of the growing bone in a crowded mass, held together by an intervening and apparently structureless matrix (Plate IX. fig. 27 *b*, *Tr*.) Here and there we find a cell which has accumulated about itself an outer investment of transparent tissue, and has in fact become developed into a lacunal cell destined to become a lacuna.[2] These points are illustrated in fig. 29 *b*.

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[1] Mr Quain and Dr Sharpey, *oper. cit.* cxlix. and page clix. Dr Sharpey says, " When further examined with a higher magnifying power, the tissue or membrane in which ossification is proceeding, appears to be made up of fibres and granular corpuscles, with a soft amorphous or faintly granular uniting matter. The fibres have the character of the white fibres, or rather fasciculi of the cellular or fibrous tissue, and are similarly affected by acetic acid. The corpuscles are for the most part true cells with an envelope and granular contents : some about the size of blood-particles, but many of them two or three times larger. In certain parts the fibres, but in most the corpuscles predominate, and on the whole the structure may be said to be not unlike that of fibrous tissue in an early stage of development." Kölliker, *oper. cit.* p. 289, confirms Dr Sharpey's statement, but denies that a fibrous arrangement can be traced in formed bone.

[2] The various views which have been entertained regarding the formation of the lacunæ and canaliculi have been concisely stated by Dr Sharpey, *oper. cit.* p. 158. He observes, that " they are generally supposed to be derived from the cells of the soft tissue involved in the ossification by some sort of metamorphosis which has been variously conceived. Some suppose that the cells become the lacuna and send out branches (like the pigment cells) to form the canaliculi (Schwann*). Others think that it is not the cell but its nucleus that undergoes this change, and that the substance of the nucleus is afterwards absorbed, leaving the lacuna (Todd and Bowman†)." The nucleus described by Todd and Bowman is identical with that which in this communication is called

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* Mikroscopische Untersuchungen. † Physiological Anatomy.

The process of growth may be thus described. In the meshes of the fibrous tissue on the surface of the bone *osteal cells* are developed and gradually take its place; a few cells become developed into lacunal cells; the earthy salts are added, and concurrently lacunæ and canaliculi are formed; we then have bone presenting the usual characters of that tissue[1]. In bone developed in the foregoing manner, we find the canaliculi not merely extending to the surface of the cell-wall, or anastomosing with the canaliculi of lacunal cells lying in contact with it, but extending freely in all directions and passing through or amongst the ossified cells, and establishing rich plexuses of anastomosis. Indeed we see the boundary of the original lacunal cells only in those cases

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the granular cell, and from which the authors have shewn the lacuna is formed. "Henle[*] thinks that the lacuna is a cavity left in the centre of a cell which has been partially filled up by calcification, and that the canaliculi are branched passages, also left in consequence of the unequal deposition of the hard matter, as in the instance of the pore cells of plants." "It rather appears to me as if the lacunæ and canaliculi were little varieties left in the tissue during the deposition of the reticular fibres, as open figures are left out in the weaving of some artificial fabrics (but not within a cell, as Henle imagined), and that thus the apposition of the minute apertures existing between the reticulations of the lamellæ gives rise to the canaliculi." "At the same time it seems not unlikely that a cell or a cell-nucleus may originally lie in the lacuna or central cavity, and may perhaps determine the place of its formation." Hassall[†] agrees with Schwann, while Gerber[‡] and Bruns[‡] appear to hold the views of Todd and Bowman. With the exception of Dr Sharpey, the above-named authorities may perhaps differ more in the use of terms than in matter of fact. The appearances represented in figs. 5 and 27, *Tr.*, would at first view seem to justify the opinion expressed by Dr Sharpey, but a careful examination of the tissue during its development, the unquestionable fact that in the development from cartilage the granular cell becomes converted into a lacuna, together with the circumstance that lacunal cells are frequently found in the Haversian canals and cancellated structure, especially in the bones of old subjects, and at times imbedded in the structure of the bone, have left no room for doubt in the authors' minds that the lacunæ are formed from special nucleated cells, in the manner described in the text.

[1] When speaking of the growth of cartilage, it was stated that the bulk of that tissue at the epiphysis increased laterally by the division of the cells, but the fact that it also increased on the free surface of the greatly enlarged cartilaginous epiphysis of the fœtus by a process similar to that by which the diameter of bones is increased, was reserved to be described in connection with the latter subject. If a longitudinal section be taken from the epiphysis of a fœtal long bone, including some portion of the perichondrium, it will be seen that the cartilage passes gradually into a more or less fibrous tissue, which forms the exterior of the part. Amongst the fibres will be found numerous elongated cells, which on tracing the specimen inwards will be seen to be similar to the cartilage cells at and near the surface of the tissue, while the fibres will be seen to give way to or become converted into hyaline tissue, as shewn in Plate IX. fig. 30, in which the cartilage is indicated at *a*, and the cells included in the fibrous tissue at *b*.

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[*] Anatomie Générale.

[†] Microscopic Anatomy of the Human Body, p. 310.

[‡] Allgemeine Anatomie.

where the lacunæ have but few, or are entirely devoid of canaliculi. It would appear to be a law, to which there are few if any exceptions, that when anastomosis is established between adjoining lacunæ, the lacunal cells blend with the contiguous parts, and are no longer recognisable as distinct bodies. The process by which the cylindrical bones are increased in diameter is in all essential points similar to that described as pertaining to the growth of flat bones. Similar osteal and lacunal cells are present, but the relative amount of the matrix is greater; moreover the osteal cells have a disposition to assume a linear arrangement corresponding to the direction of the laminæ of the contiguous bone. In these lines the cells are placed so close to each other as to leave but little room for intervening tissue, but between the lines an appreciable amount may be recognised (Plate IX. fig. 31, *Tr.*) This appearance however varies in different specimens. In one the cells predominate, in another the transparent tissue is the more abundant. Generally the younger the animal the greater will be the amount of the intervening transparent tissue, and the smaller the number of the osteal cells. But in all cases, whatever the age of the subject, or from whatever part of the skeleton the specimen be taken, the cells and the intermediate tissue become blended in the process of ossification, and the whole presents a uniform granular appearance, excepting in the instances in which lamination is strongly developed, or in those which have been noticed in the previous part of the paper. We frequently find portions of bone where the osteal cells, lacunal cells, and intermediate tissue are so perfectly fused together that neither can be recognised, but in their place we have a minutely granular mass, divisible only into lacunæ and canaliculi and the tissue in which they lie imbedded. In Plate IX. fig. 31, *Tr.*, taken from the tibia of a fœtal lamb, the osteal and lacunal cells have become blended to a considerable extent, but without the occurrence of lamination. The lamina which is represented at the margin is composed of still uncalcified cells. It shews the mode in which the undulating laminæ are formed. Fig. 33, *Tr.*, also, taken from the fibula of a calf, shews ossified lacunal and osteal cells without any tendency to lamination.

The manner in which the Haversian spaces become gradually occupied by Haversian systems is peculiarly interesting. To obtain a good view of the process, it is necessary to make a transverse section of the developing systems. It may then be seen that osteal cells arrange themselves in single file within the Haversian space, with intermediate lines of transparent tissue, and here and there a lacunal cell;

the process commencing at the surface of the Haversian space, and extending gradually inwards till the system is completed. In fact the soft tissue takes the permanent form previous to the addition of the salts of bone, much in the same manner and to the same degree as occurs in temporary cartilage before the earthy ingredients are deposited. Lamination is nothing more than a definite linear arrangement of the osteal cells with their outlines permanently retained in the perfected bone; a character much more strongly marked in the bones of adult than in those of young animals.

In pursuing their inquiries into the growth of bone, the authors found it necessary to take sections of perfectly fresh bone, and to examine them in albuminous fluid. Spirit of wine, whether diluted or not, obscures the normal appearances, and water is not more favourable as a medium.

Sections permanently mounted in dilute spirit lose a good deal of their character; still the appearances are preserved with more or less of their original distinctness. Sections mounted in Canada balsam shew some of the points remarkably well. For instance, the partially ossified osteal and lacunal cells are tolerably well preserved, but unfortunately those cells which have not received any of the indurating salts are represented only by a transparent mass, in which but little structure can be recognised; thus, the part *b* in fig. 32 would have appeared but as a transparent line, had the specimen been mounted in Canada balsam. In addition to the necessity for care in the examination of osseous structures, under the most favourable circumstances, as regards the selection of specimens and the fluid used in their preparation, it is equally necessary to have the more recent appliances for the illumination of the objects.

Mr Gillett's achromatic condenser, with what is called the white-cloud illuminator, renders it very easy to demonstrate points which with the ordinary microscopic apparatus are shewn with difficulty. It need not, however, be urged, that it is desirable, in studying structural anatomy, to avail ourselves of the most perfect instruments that can be obtained. It is proper that the authors should, before leaving this part of the subject, draw some comparison between the development of bone in cartilage and in the softer tissues.

Temporary cartilage, previous to the development of bone, affords a mechanical support and protection to the surrounding or inclosed softer tissues. These offices could not be rendered by a mere aggregation of soft cells, but are efficiently performed by the dense intercellular substance, which forms so characteristic a feature of the cartilage. Hence, in the mechanical function of cartilage, the intercellular element of the tissue must be regarded as the most important.

A second, and scarcely less important purpose effected by temporary cartilage, is that of affording a medium for which a more solid tissue may be substituted, without the mechanical support being withdrawn from the adjoining parts during the process of change. It affords also a means by which the long bones are gradually increased in length, without any interference with the functions of the limb. These changes are brought about by the gradual increase in the number of the cartilage cells, at those parts only where ossification is about to commence, and by the conversion of the cells into lacunal cells at the cost of the intercellular tissue, which, whilst its bulk is diminishing, becomes impregnated with the earthy salts ; so that although the quantity is lessened, the strength of that which remains is increased. Now, in examining the process by which growing bones are enlarged, it must be borne in mind that the mechanical function of the cartilage is performed by the bone which has replaced it, and that no such office is required of the new tissue which is gradually adding to the bulk of the bone. The necessity for a dense intercellular tissue no longer exists. Hence we have an aggregation of osteal cells with so much only of intercellular tissue as will serve to connect them into a mass. A certain number of these cells, by additions to their exterior, become lacunal cells. We have then in the place of lacunal cells and dense intercolumnar tissue, lacunal and osteal cells united by a small and almost imperceptible amount of a semifluid intercellular medium; the relative proportions of these several parts varying a little with differences of age. Thus, in aged subjects, aggregations of lacunal cells may here and there be found with but few osteal cells, and but little intercellular tissue, while in a young subject osteal cells will preponderate.

In all cases, a direction may be given to the cells of a growing part, by their being in relation with the insertion of a tendon.

A consideration of the foregoing facts will, we think, lead to a conviction, that the two different forms in which bone is developed, are designed to meet the requirements of the animal at different periods of life, and that bone developed by means of osteal and lacunal cells is the higher form of the tissue. In early embryonic life, there is formed a soft cartilaginous skeleton, which is hard as compared with the surrounding parts, and capable of affording them the required amount of mechanical support. As the various tissues advance in development, the cartilage increases in density by the elaboration of its hyaline tissue, and after a while those changes occur, which precede its conversion into bone. But in the bone so formed, it has been shewn that the canaliculi anastomose only at those points where the lacunal cells come into contact. (Plate VIII. fig. 25, *Tr.*) In bone formed from osteal and lacunal cells, the anastomosis of the canaliculi prevails throughout the tissue, while the outline of the formative cells is almost entirely lost in the general blending of the whole into a subgranular mass, subject to lamination in the manner already described.

It will, therefore, be in accordance with the preceding views, to regard the intercolumnar tissue present in primary bone, rather as an accessory element calculated to give support during the ossification of the lacunal cells, than as an integral and necessary element of osseous tissue; for it has been shewn that it offers an obstruction to the anastomosis of the canaliculi, and that the lacunal cells preserve their outline so long as they are inclosed within it, instead of becoming lost in the surrounding tissue, as occurs in secondary bone. It must not, however, be forgotten that small patches of ossified intercolumnar tissue may here and there be found between the Haversian systems, even in the bone of old subjects; (Plate VII., fig. 16, *Tr.*); so that its presence in a small amount (as accidentally left when the Haversian spaces are forming) is not incompatible with the normal condition of perfect bone.

It may also be stated in this place, that the undulating laminæ formed on the surface of growing bone, although for the most part removed to make way for Haversian systems, are also found here and there in small patches in the bones of adults.

The subject of absorption, as it relates to the removal of bone, has already been partly discussed; but something more remains to be said. Although the process of absorption has not, and probably cannot be seen in actual operation, yet a consideration of the relative position of the increasing and wasting parts, and of their conditions, will, in the authors' opinion, justify the conclusion, that the bone is removed through the agency of cells.

In seeking to find the circumstances under which the absorption of bone takes place most rapidly, and to the greatest extent, the authors have been led to examine bones which had been placed in various conditions, both of health and disease. It has been already stated that bone developed in cartilage is speedily removed; but it does not seem that bone formed by osteal and lacunal cells is absorbed with equal rapidity although in the course of a short time it not less surely disappears. This difference in the rate of absorption is probably the result of the vascularity and higher state of development of the latter tissue. In bone developed in cartilage, Haversian spaces have to be formed before it can be permeated by vessels; hence in this tissue absorption proceeds rapidly, the process being established as soon as the bone is formed. In adult bone, when in a normal condition, we find here and there an Haversian space; but if a bone around which the soft parts are in a state of inflammation be examined, numerous Haversian spaces will be seen. If we examine the same bone in the neighbourhood of that part where the inflamed is merging into the healthy investing texture, we shall find new osseous matter in the process of deposition on the surface of the pre-existing bone. Examples of this may frequently be found in the vicinity of diseased joints.

We have here an instance of the development of new tissue, and the removal of a pre-existing one being set in operation, by what would appear to be a different degree only of the same action. Hence a more particular examination of the cellular mass, by which bone appears to be absorbed in the one case and deposited in the other, naturally suggests itself, and attention has been directed to this point as frequently as favourable opportunities for observation have presented themselves. The differences, however, in microscopic characters between the two tissues, have not been so strongly marked, as to admit of any definite description being given, by which the one may, in all instances, be distinguished from the other, when removed from their natural positions, as is almost necessarily the case with regard to the absorbent cells, from the readiness with which they separate from the wasting bone (Plate IX., figs. 27 and 34, *Tr.*)

Lacunal cells, moreover, will not be found in the absorbing tissue; but it is not easy to recognize them, even in the tissue from which bone is developing, as they are frequently obscured by the osteal cells; so that their presence, although a good ground of distinction, cannot in all instances be demonstrated. When, however, developmental cells are seen *in situ*, their character is readily distinguished. They are closely applied to the surface of the increasing bone, which loses the festooned and assumes an even outline, to which the osteal cells adhere.

The source from which the cells destined for effecting absorption arise, is at present unknown to the authors, any further than that they arise in connection with soft tissues, the three situations in which they occur being beneath the periosteum or medullary membrane, or within an Haversian canal. These are, no doubt, the points from which the development of the cells starts; but the law which regulates their occurrence at one part rather than another, and gives to them their peculiar function, is at present unexplained. Neither is it more easy to understand why, at a particular period, the process of absorption is arrested and development of new tissue commences; the new bone may, for anything we have seen to the contrary, be formed from the same cells that have been concerned in the removal of the old. The organ by which the fangs of teeth are absorbed, offers a very favourable object for examination.

This we find to commence within or beneath the periosteum which covers the fang of the tooth, and increases with the wasting of the tooth, until it comes in contact with the pulp, which then assumes a similar function and becomes an absorbing organ, increasing gradually in size, till, if the tooth be left undisturbed, but little of the crown remains excepting the enamel. If, when in this condition, the crown of the tooth be carefully removed, we shall see the absorbent papillæ projecting a little above the gum, firm in substance, and not disposed to bleed unless rudely handled. Within two or three days it becomes covered with epithelium, either by the extension of the epithelial membrane from the surrounding parts, or from a change in character in the superficial cells of the part itself.[1] Gradually the papilla loses its distinctness of outline, and merges into the surrounding gum, from which it can no longer be distinguished. Here, then, we have a second instance in which an absorbent tissue has its function finally suspended, and a new one substituted. In the place of increasing at the cost of pre-existing tissue, growth is suspended, and it unites with and assumes a similar office to that of the surrounding gum.

In conclusion, the authors may allude to an interesting fact, with reference to the subject of absorption, which has been recently observed by Mr Stanley, and subsequently by Mr Bowman. In the treatment of ununited fracture, the practice has of late been successfully adopted of drilling cylindrical holes into the bone in the neighbourhood of the injury, and of driving in pegs of ivory, accurately fitted to the perforations, for the purpose of setting up action in their vici-

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[1] In the 26th volume of the Medico-Chirurgical Transactions, there is a short paper by Mr Dalrymple, in which he describes a small tumour from the eyelid, consisting essentially of epithelial cells, in which the ordinary contents had been replaced by ossific matter.

Fig. 1.

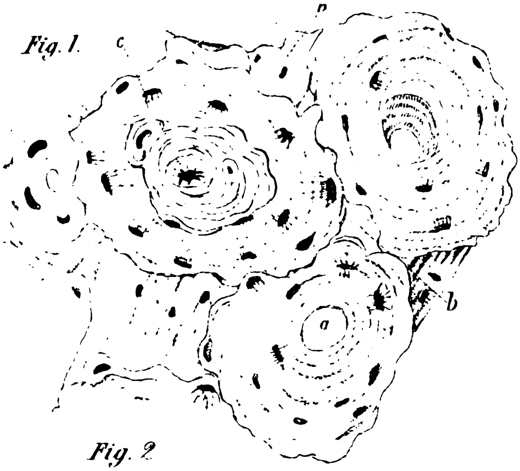


Fig. 2

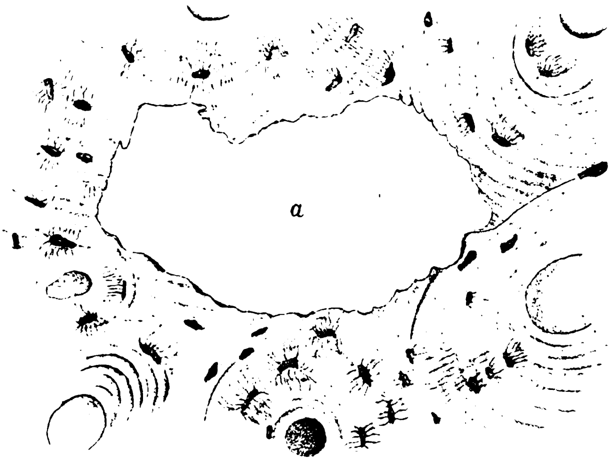


Fig. 4

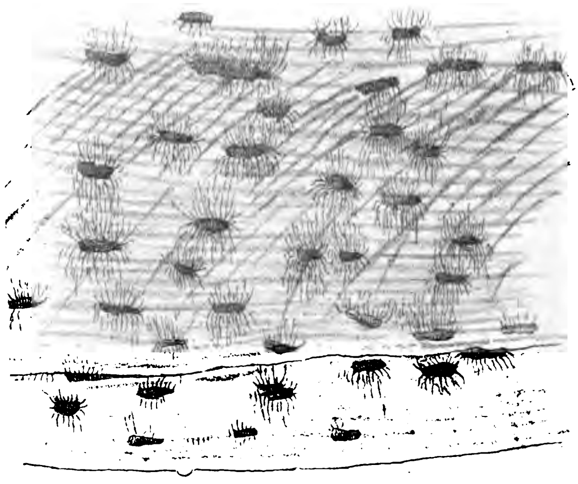


Fig. 3.

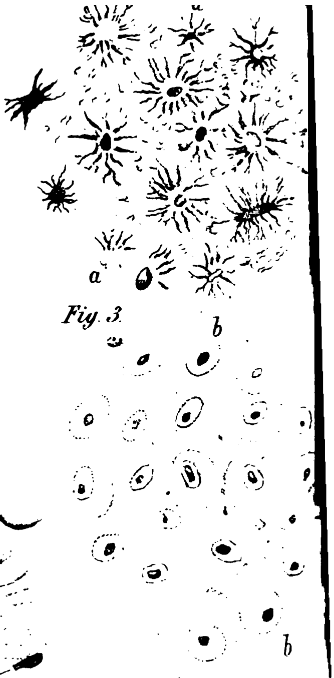


Fig. 5

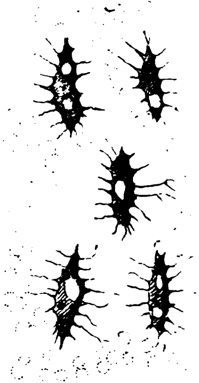
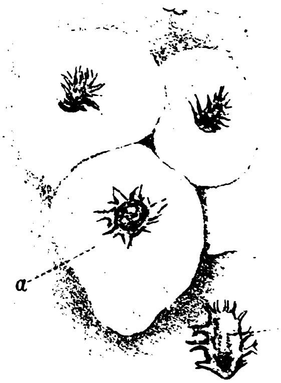
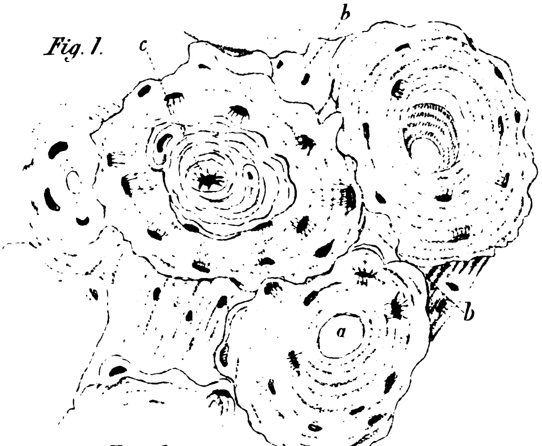


Fig. 6



Schenck & McFarlane.

Fig. 1.



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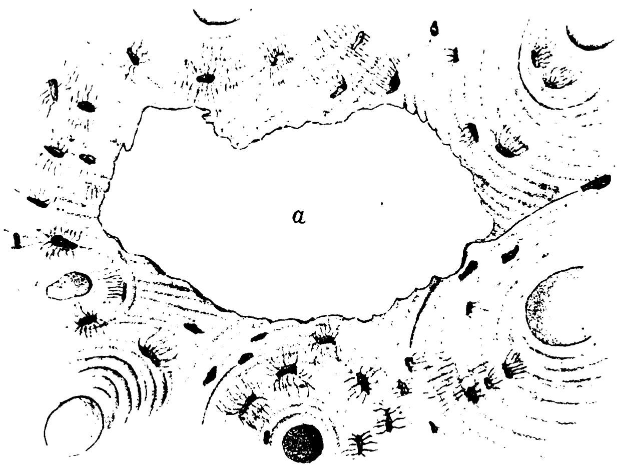

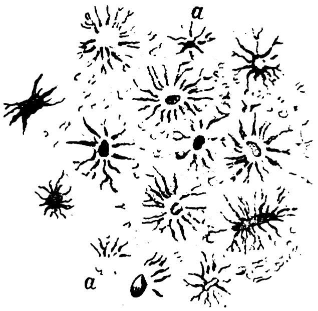

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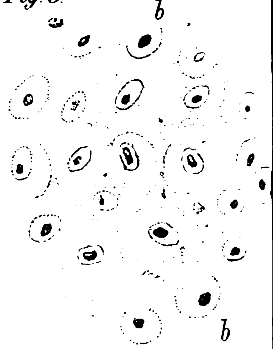
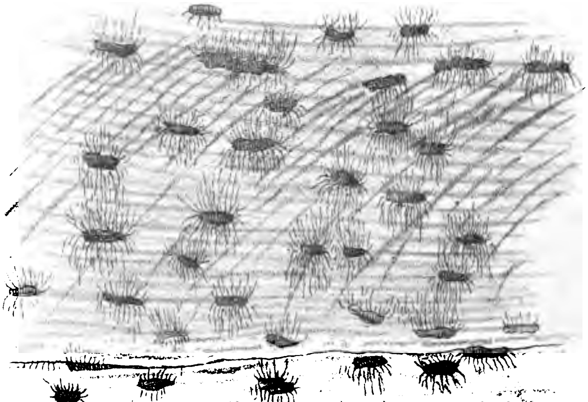


Fig. 4.



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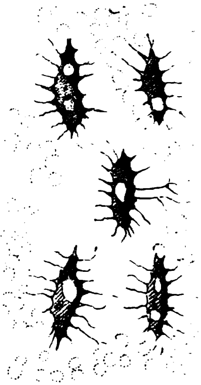

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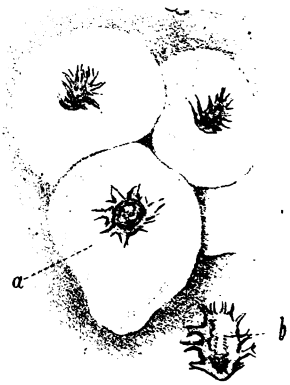

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nity. These pegs, after remaining in the bone several weeks, have been removed, and have been found on inspection to present erosions similar to those which are seen on absorbing bone or tooth. In fact, they closely resemble the fangs of temporary teeth recently attacked by absorption. Mr Bowman kindly placed at the disposal of the authors one of the pegs which had been used for this purpose, a section of which presents, at the parts eroded, in its microscopic characters, the usual emarginated outline so frequently alluded to in the foregoing pages. They have not, however, had the opportunity of investigating the condition of the tissues around the pegs; but it is exceedingly probable that absorption has, in these cases, been effected by processes similar to those which occur in the formation of Haversian spaces.

Explanation of the Plate.

Fig. 1. Transverse section of compact bone, shewing the ordinary appearances. *a.* Haversian system. *b.* Interstitial laminæ. *c.* A new Haversian system within an older one, the Haversian canal obliterated by the development of a lacunal cell.

Fig. 2. Transverse section of compact bone, shewing an Haversian space, with its characteristic emarginated outline.

Fig. 3. Sections from rickety bone. *a.* Consolidated lacunæ. *b.* Lacunal cells, from which no canaliculi have proceeded.

Fig. 4. Section shewing circumferential laminæ, with elongated canals passing amongst the lacunæ and canaliculi.

Fig. 5. Section of fossil bone, from the Wealden, supposed to be Pterodactyle, in which the nuclei have been preserved by fossilization.

Fig. 6. From rickety bone. *a.* Lacunal cells. *b.* A detached granular cell in process of conversion into a lacuna.[1]

ART. IV.—*Permanent Cure of Reducible Hernia.* By GEORGE HAYWARD, M.D., of Boston, Massachusetts. (Presented to the American Medical Association, at its Session of May 1852.)

The undersigned, a Committee of the American Medical Association on the Permanent Cure of Reducible Hernia, beg leave respectfully to report:—

That they addressed a number of questions to their professional brethren throughout the country, in the hope that they might in this way gain such information on the subject of their commission as they would not be likely to obtain in any other. They regret, however, to be compelled to state that they have received but seven answers, and that, though some of these are interesting and valuable, they still do not throw so much light upon the point to be investigated as they had hoped to derive from this source.

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[1] This paper is illustrated with Four Plates containing thirty-four Figures. From these, six of the most important and characteristic are selected. The references to the original figures are allowed to remain.

The first of them, from Royal A. Merriam, M.D., of Topsfield, Massachusetts, is occupied by the details of an operation for femoral hernia, in which a portion of the omentum was removed. The patient recovered, and lived twenty years without any further protrusion. It is not stated whether a truss was afterwards worn or not.

It seems to have been a common case of operation for strangulated femoral hernia, in which the omentum alone protruded. This operation, it is well known, sometimes effects, though very rarely, a permanent cure. It is also well known that the hernia much more frequently returns after it, unless other means are adopted to prevent it.

The second answer is from George W. Hinman, M.D., Derby. It gives the history of a case of reducible inguinal hernia, in which the sac was laid open, and the inside of it "brushed over with the tincture of iodine," the contents of the sac having been, of course, previously returned into the abdomen. The patient did well, and there had been no return of the hernia at the time the letter was written—a year only after the operation—a period not sufficiently long to enable any one to say that the cure would be permanent. It is not stated, either, whether the patient continued to wear a truss.

The writer of the third answer, George O. Pond, M.D., of Griggsville, is confident that he has effected a cure, by means of pressure, in a number of cases of inguinal hernia. Stagner's truss is the instrument by which the pressure has been applied; and of twenty cases he thinks that seven-eighths have been permanently cured. That pressure is one of the most important modes of treating hernia, with a view to a radical cure, there is no doubt; and upon this point the Committee will take occasion to speak hereafter. Though there is nothing new in this communication, it is nevertheless valuable, as it affords additional evidence of the power of one of the means employed to remove this infirmity.

The writer of the fourth letter, George Heaton, M.D., of Boston, has devoted himself pretty extensively to the treatment of reducible hernia, and had given notice to that effect from time to time, for some years, in medical and other publications. It came to the knowledge of your Committee that two or three individuals regarded themselves as cured by his treatment, and, as he was a member of the profession of regular standing, it was supposed that he would cheerfully state his peculiar method, if any such he had, of managing such cases. The Committee, therefore, sent him not only a copy of the questions which they addressed to the profession at large, but they also wrote him a private note, couched in the most respectful terms.

To this he made a courteous reply, but at the same declined giving the information sought for. Not content with this, he caused the note addressed to him, and his answer, to be printed in several newspapers, which has, in our opinion, given him a notoriety, wherever the transaction is known, of a very unenviable character. It is certainly an unusual course for a member of our profession to conceal from his brethren any method of treatment which he may regard as more valuable than those in common use, and it is clearly one which cannot be too strongly reprobated by all honourable and high-minded men.

The fifth communication, from John Watson, M.D., of New York, was an account of an operation for the radical cure of an inguinal hernia by injection. The writer adopted the subcutaneous method, as recommended by Dr Pancoast.

The result was favourable, but a sufficient length of time had not elapsed to enable any one to say that the beneficial effects will be permanent. The operation was done about ten weeks before the letter was written.

A part only of the sixth answer, from Professor Alden March, of Albany, was strictly in reply to the questions proposed by the Committee. Much the larger portion was occupied by a history of eleven cases of strangulated hernia, for the relief of which an operation was performed, and in eight of these the writer believes that a radical cure of the hernia was the consequence.

The remaining portion contains an account of four cases of reducible hernia, in each of which the writer performed the subcutaneous operation by injection, but in none of them was a radical cure produced.

The seventh and last communication, from Professor Paul F. Eve, of Augusta, Ga., gives an interesting history of two operations, on a boy of ten years of age, for reducible inguinal hernia, supposed to be congenital. The first operation was that of Bonnet, by means of pins, which was unsuccessful, and, at the end of a month, "a portion of the scrotum was invaginated." After adhesion took place, one of Chase's trusses was applied. The patient continued well for eight months after the last operation, and the operator thinks that he has reason to believe that the cure will be permanent.

This, from the age of the patient, would be very likely to be the result of any method which should prevent, for a year or more, the descent of the hernia.

While the Committee would express their grateful acknowledgments to the individuals who have made the communications that have just been referred to, they at the same time do not feel that all the information has in this way been obtained that the Association have a right to expect. They have, therefore, looked to other sources in addition, to aid them in the preparation of their report; and they will now state, as briefly as they can, what has been done in relation to the radical cure of reducible hernia, and the opinions they have formed on the subject.

It is hardly necessary to go into a detailed history of the various methods that have been employed for the last eighteen hundred years for the permanent cure of reducible hernia, and the opinions they have formed on the subject. A very interesting and condensed account of them may be found in a dissertation, by Henry Bryant, M.D, of Boston, on "The Radical Cure of Inguinal Hernia," published during the present year, and for which the Boylston Prize was awarded to the author in the year 1847.

All the operations that have been practised for this purpose, till within the last fifty years, have been of a severe character; some of them dangerous, and in many instances death has been the consequence. *Cauterization*, *ligature*, *sutures*, *excision* of a *part* or the *whole* of the *sac*, and *castration* were the principal operative methods in use for eighteen hundred years. The object of all of them was to obliterate or contract the neck of the sac, and thus prevent the protrusion of any of the abdominal contents.

Cauterization.—The caustic was applied to the skin just over the external ring in inguinal hernia, and immediately below Poupart's ligament in femoral. Sometimes the actual and at others the potential cautery was used. It was not unusual to make several applications of the caustic, so as to destroy the skin, sac, and periosteum in inguinal hernia, and thus produce an exfoliation of the bone. When the eschar separated, the part was dressed in the same way as a common ulcer. This was the method adopted by the ancients in the performance of this barbarous operation. Monro, and some other practitioners of modern times, modified it by making an incision in the first instance, and exposing the neck of the sac, and then applying the caustic. They thought that they thus increased the chance of success, and at the same time lessened the danger.

But even when modified in this way the most disastrous results often followed it. The spermatic cord was not unfrequently destroyed; the intestine was sometimes perforated; and in many cases death ensued, either from violent and extensive sloughing of the scrotum, attended with severe constitutional irritation, or from peritoneal inflammation. And, in addition to all this, in several instances in which the patient survived this severe treatment, the hernia reappeared.

It is hardly necessary to add that no one at the present day could probably be found to justify, much less perform, such an operation. It is not, perhaps, surprising that the ancients, in their anxiety to relieve an infirmity which was so common, and over which, from their ignorance of anatomy, they had so little control, should resort to almost any method that held out a tolerable prospect of success: but it is wonderful that, after the repeated failures that followed it, as well as its often fatal results, it should have continued in use till modern times.

Ligature.—Ligatures were applied in different ways. One of the earliest methods was to pass a string around the integuments over the neck of the sac, its contents having been previously returned into the abdomen, and tying it so tightly as to cause all the parts below to slough. In inguinal hernia in the male, this, of course, destroyed the testicle.

Another mode was to pass a needle with a ligature through the skin and around the neck of the sac, and then tie the ends of the ligature over a piece of wood placed on the integuments. The pressure could in this way be increased till sloughing was produced; and it was pretended by some who practised this operation that, if proper care was used, the sac only might be included in the ligature, and thus the testicle be saved. This, however, did not prove to be true; it was found that notwithstanding the utmost precaution, the spermatic cord was so much compressed that the functions of the testicle were lost, even if the organ itself was not destroyed.

In order to obviate this difficulty, the operation by the "punctum aureum," as it was called, a ligature of golden wire, was suggested and practised. An incision was made in the integuments, the sac laid bare, and a needle, armed with a wire of gold, was carried under the neck of the sac near the external ring. The ligature was tightened from time to time, and it was thought that the neck of the sac might in this way be sufficiently closed to prevent the return of the hernia, without destroying the spermatic cord. But such was not the fact.

The same operation was performed with a common ligature, and sometimes with one of lead. But in all these methods, so much pressure was made on the spermatic vessels as to produce an atrophy if not an entire destruction of the testicle; and this circumstance, probably more than anything else, led to the operation by

Suture.—This method, because it did not destroy the virility of the patient, and would not consequently deprive him of the power of increasing the king's subjects, was called "the royal stitch."

There were two modes in which this operation was performed. The first step in both was to return the contents of the hernial sac. This being done, in one method a needle armed with a ligature was passed through the neck of the sac, which was sewed up by the continued suture, as is done in wounds of the intestines. No incision was made, but the needle was carried through the integuments.

In the other method, the hernial sac was first laid bare, the continued suture was used in the same way, and then a part or the whole of the sac below the suture was removed. In both cases the thread was allowed to remain till it was thrown off by the process of ulceration.

This operation could only be used in inguinal hernia. It was by no means uniformly successful; it was difficult to perform, when the sac was to be dissected away; always severe and dangerous, and oftentimes followed by death. For these reasons it is not practised at all at the present day, but is reprobated by all modern surgeons who have had occasion to speak of it.

Excision.—Excision of a part or the whole of the hernial sac was another of the operations that was in practice till comparatively modern times, for the radical cure of reducible inguinal hernia. When the first method was adopted, the contents of the sac being previously returned, a portion of the covering of the hernial tumour, including the integuments and hernial sac, was cut away just below the external ring.

The parts were then brought together and allowed to heal; and it was thought that such a contraction would be produced in the mouth of the sac as would prevent any further protusion. This, however, was rarely the case. The operation, too, was necessarily a severe one; fatal peritonitis frequently followed it, and in many cases death ensued from a wound of the viscera, which adhered, as they often do, to the neck of the sac.

The removal of the whole or the greater part of the sac was a still more severe and dangerous method, and at the same time equally uncertain. Probably because fatal hæmorrhage was by no means an unfrequent consequence of this operation, a modification of it was practised, but with very little, if any more success. This consisted in making an incision from the neck to the bottom of the sac, and then removing a portion of it on each side, the whole length of the incision.

Another operation, that was practised to a great extent and for a long period, though not so dangerous, but far more unwarrantable than that of excision was *castration.*

This was done occasionally with the knowledge of the patient, but more frequently without. Till within a very recent period, most persons afflicted with hernia, especially inguinal, have been anxious to conceal the fact, from the erroneous opinion that it impaired, if it did not destroy their virility. They were willing, therefore, to submit to almost any severe method of treatment that would relieve them of their infirmity. But it is hardly to be supposed that they would consent to the removal of one of the genital organs, because they apprehended a loss of its functions, any more than that they would commit suicide to escape death.

It is well known that various operations were performed for the radical cure of reducible hernia by ignorant persons, oftentimes unprincipled charlatans, who were not very scrupulous as to the means they adopted. Finding that their other methods failed very frequently, and in many instances caused death, they thought that castration would be more likely to produce a cure, and at the same time be far less dangerous. They pretended only to make a free incision in the sac, and declared that, when the wound was healed, the neck of the sac would be so much contracted that a permanent cure would be the consequence. That most of these operators had no confidence in this statement, is apparent from the fact that, after making the incision, they proceeded to divide the spermatic cord and remove the testicle; and such was the ignorance of many of them, that in numerous cases the patient died from the loss of blood. To such an extent was this barbarous operation carried at one time, that in some parts of Europe, penal laws of the severest character were enacted to prevent it, and its performance for the cure of hernia was in some countries made a capital offence.

It was done oftentimes by uneducated, itinerant operators, some of whom not unfrequently were females, not only for the cure, but for the prevention also of hernia, and for this purpose thousands of children have been subjected to this mutilation.

The danger of the operation and its frequent want of success may perhaps be in some measure attributed to the unskilfulness of the operators; but it often failed of effecting a cure, and sometimes caused death, when practised by the most expert. Notwithstanding all this, and in defiance of the law, there were found individuals who continued to practise it for this purpose till within the last sixty years.

It is certainly remarkable that inguinal hernia should return so frequently as it is said to have done after castration; but we have the authority of Sabatier for saying that it was not more effectual in removing the difficulty than the application of caustic, the golden ligature, and some of the other methods that have been already noticed.

This fact should teach us not to place too much reliance on any of those operations that are intended to produce a partial or complete closure of the neck of the sac; for when the testicle was extirpated for the cure of hernia, the whole sac was sometimes removed by passing a ligature around it, just below the external ring, which caused the parts below to slough, and yet the hernia occasionally again protruded.

The severity, danger, and frequent failure of these operations at length caused all of them to be abandoned; and Mr Lawrence said, with great justice, in his Treatise on Hernia, more than five and thirty years ago: "Since the enlarged state of the tendinous opening is not removed by the processes adopted for a radical cure; since a recurrence of the disorder is not prevented, we may assert without hesitation that these operations do not afford any greater chance of complete relief than the employment of a truss."

The Committee will next inquire whether any of the numerous methods that have from time to time, since that period, been suggested and put in practice, have been attended with a greater degree of success. They will not go into a minute detail of all of them, but will present such a general view as will, they trust, make apparent the ground on which their opinions rest.

It should be remarked that none of the modern operations for the radical cure of hernia are as severe as the worst of the older ones of which we have already spoken; though some of them, it must be admitted, are very objectionable on this account.

It should also be observed that the mere fact that so many have been suggested and tried, and that none of them have been received with a great degree of favour, or, if so, that they have not retained it for any considerable length of time, is strong presumptive evidence that there is either an inherent difficulty in the nature of the infirmity that is intended to be removed by them, or that the means of accomplishing it have not yet been discovered.

It is well known that most of these operations have been devised for the radical cure of inguinal hernia, as this is the most frequent form, and the one which is, on the whole, productive of the greatest inconvenience. The object has been to close either wholly or in part the neck of the sac, or plug up, without contracting, the tendinous openings through which the hernia escapes. It has been attempted to accomplish both these in various ways.

In some operations that have been performed for omental hernia, whether strangulated or merely irreducible, a portion of the omentum has sometimes been left in the inguinal canal, under the belief that the adhesions which it would form with the surrounding parts would prevent any future protrusion. Several surgeons have reported cases as successfully treated in this way; but it is not stated for how long a time the patients enjoyed this immunity, and it cannot therefore be known whether the cure was permanent.

One of the Committee is able to state a case which came under his own observation, which, in his opinion, has an important bearing on this point. A healthy young man underwent an operation for irreducible omental hernia. All the omentum beyond the external ring was cut off; the inguinal canal was plugged up by the omentum, which was closely adherent to it; and it was remarked at the time that the patient would never probably be troubled with hernia again. Within two years of that period, he was obliged to submit to an operation for strangulated enterocele on the same side. The very circumstance that was supposed to be sufficient to relieve him permanently was probably the cause why the hernia could not be returned by taxis.

It has been attempted to close the external ring by forcing the testicle into it, and then bringing on such a degree of inflammation as would be likely to retain it there by the effusion of fibrin. Sometimes this has been done without making an incision in the integuments, and at others the testicle has previously been laid bare.

This operation is certainly not admissible. It is unsafe; it is not probable that the testicle could be kept either at the external ring or within the inguinal canal; if it could, it would, no doubt, be a source of great inconvenience and irritation; and in addition to all this, it would not in all probability prevent the return of the hernia, but would, on the contrary, rather facilitate it.

It has been attempted also to close the external ring and inguinal canal by means of the hernial sac. This operation has been done by Petit and Garengeot. The sac is first exposed; a portion of it is then crowded into the inguinal canal, under the expectation that adhesions would be formed between them sufficiently strong to prevent any subsequent descent of the hernia. This method is, perhaps, less objectionable than the preceding one, on the score of danger to the patient, though by no means entirely free from it, but is not any more likely to effect a cure. There is no reason to believe that it is performed by any one at the present day.

M. Gerdy has practised an operation, which consists in crowding the integuments into the inguinal canal, and removing the cuticle from them by means of caustic alkali. This is what has been called by the French "invagination by the integuments." Some modifications of this method have been suggested by M. Leroy and M. Signorini, but from what we have been able to learn, we do not deem it of sufficient importance to give any detailed account of it.

M. Velpeau states that he performed it once unsuccessfully; that it had been done by M. Gerdy upon thirty patients; and that, though many of them seemed for a time to have been cured, a sufficiently long period had not elapsed to enable any one to speak with confidence of the ultimate result. He also adds that he had seen three of the persons who had been thus operated on, and who thought for some time after the operation that they were cured, in all of whom the trouble had returned precisely as it was before.

The truth seems to be that the adhesions which may be formed between the integuments and the interior of the canal are not very firm, and that, though they may for a time prevent the descent of the hernia, yet, as they are gradually absorbed, no resistance is at length offered to it.

M. Velpeau observes that, though it is not actually a dangerous operation, there is some risk of wounding the epigastric artery, and there is some reason, too, to fear severe phlegmonous inflammation or fatal peritonitis.

M. Belma's operation attracted for a time some degree of attention, as it was less severe than that of M. Gerdy, and would, it was at first thought, be in all probability more successful. His first method consisted in introducing a small pouch of goldbeater's skin into the upper part of the hernial sac. This was followed by an effusion of fibrin, which he supposed would, together with the goldbeater's skin, become organized, and the two sides of the sac being firmly glued together must necessarily prevent any subsequent protrusion of the abdominal contents.

He afterwards modified his operation, because he found that his success did not equal his expectations. This he attributed to a deficiency of inflammation; and with a view of increasing this, he introduced into the neck of the sac, as near the external ring as possible, small rolls of gelatine covered with goldbeater's skin. There is no reason to believe that the result of this proceeding was any more favourable than that of the other. The operation has fallen into disuse. M. Velpeau says that the hernia returned more frequently after it than it did after that of M. Gerdy.

Dr Jameson of Baltimore has given a description of an autoplastic operation which he performed on a female for crural hernia. The Committee have not seen the original account of the case, but have derived their knowledge of it from the statements that have been made in the works of others.

The sac was laid open, and the crural ring was filled up by a portion of the integuments, which were cut into a proper form and inverted. This was confined in its situation by sutures. Adhesion is said to have taken place, and the patient was regarded as cured,

Admitting that there was no danger in the operation, it is certainly not probable that any union would take place between the integuments and the ring that would be permanent; on the contrary, it can hardly be doubted that the fibrin effused in the first instance would be gradually absorbed, and that a protrusion of some of the abdominal contents would ere long again take place.

It is believed that this operation has not been repeated; or, if it has, the result, so far as we know, has not been made public. This fact would perhaps justify the inference that the relief obtained by the patient on whom Dr Jameson operated was not permanent; otherwise, it would be difficult to explain why a similar operation should not have been adopted in other instances.

M. Graefe has described a very barbarous mode he took to bring on inflammation in the inguinal canal, with a view to the radical cure of hernia. It has no advantage over the old and justly-reprobated operation of the excision of the sac, while it is almost as severe, and for more dangerous. The method practised by M. Graefe consists in laying bare the neck of the sac at the external ring, cutting it off at that place, and then introducing a piece of lint, smeared with some stimulating ointment, into the inguinal canal, carrying it up to the internal ring, or even beyond. One end of a piece of string is to be tied around the lint, and the other end is brought out at the wound. When suppuration is well established, the lint becomes loose and can be readily withdrawn. This is said to take place usually in three or four days, and the amount of inflammation that is induced would be sufficient, it was thought, to prevent any future descent of the hernia.

This operation has been performed, it is said, with success in a few cases; a result certainly not to be looked for, and the expectation of which would not justify any man, who had a proper regard for human life and his own reputation, to repeat it.

The introduction of a seton has been recommended as a likely means of closing the neck of the sac. It has been advised to keep it in till suppuration comes on, and then withdraw a part of the threads daily, till all of them are removed.

It was supposed that in this way adhesion might be produced between the opposite sides of the sac.

It is not probable, from what we know of its effects in hydrocele, that this would succeed; and it is much more probable that it would bring on such a degree of inflammation in the peritoneum as would terminate in death. From what is known of the laws of inflammation, there is much more reason to suppose that the suppurative process rather than the adhesive would be induced by this long-continued irritation; so that, while the patient would be exposed to great hazard, he would have but a small chance of a cure.

M. Bonnet, an eminent surgeon of Lyons, has attempted to close the neck of the sac by exciting inflammation in another way. From two to four pins, with double the number of small pieces of cork, are all the instruments that are required for his operation. The contents of the hernial sac having been returned, a pin which has been passed through one of the pieces of cork is then pushed through the integuments and the neck of the sac, as near as possible to the external ring, care being taken not to wound the spermatic cord. The pin is then brought out on the opposite side, and the point is carried through another piece of cork. Another pin is then introduced in the same way. Two are usually all that are necessary; but occasionally one or more additional ones may be required. The point of the pin which projects from the cork is seized with a small pair of pliers, and bent over so as to bring the opposite sides of the sac into close contact. This is done to all the pins, and this process is repeated from day to day, till it is thought a sufficient degree of inflammation is produced to cause adhesion. When this has taken place, the pins should be removed, and this is usually in from six to twelve days.

M. Mayor, of Lausanne, has modified this operation by using needles instead of pins, carrying in this way ligatures through the neck of the sac, which are afterwards tied over pieces of sponge. These can be tightened as much and as often as may be thought necessary to produce the desired effect. The number of ligatures required for this purpose must depend on the size of the hernial sac.

These operations are not attended with much danger, and are by no means difficult to perform. At the same time they offer but little prospect of a successful result. They are insufficient for the purpose for which they are intended. They do not obstruct in any degree the inguinal canal, and even when most successful, they only partially close the neck of the sac.

For these reasons, probably, if not for others, they have fallen entirely into disuse, not being resorted to even by the inventors of them.

Acupuncture has been tried to a very considerable extent both in Europe and this country. Two or three rows of punctures were made through the integuments and the neck of the sac, just below the external ring, with a common needle of the ordinary size, or an acupuncture needle prepared for the purpose. There is no reason to believe that any permanent good effect has been produced in this way, and it is not probable that any one tries this method at the present time with the expectation of producing by it a radical cure of hernia.

The same may no doubt be said of the *scarification of the inguinal canal*, as practised by M. Velpeau a few times, and the subcutaneous *scarification of the neck of the sac*, as performed by M. Guerin. Besides the utter inefficiency of these operations, there is, especially in the former, some danger of wounding the epigastric artery.

The operation by *injection* has been done in two ways. In one, the neck of the hernial sac is previously laid open, and the fluid then thrown in; and, in the other, it is introduced by the subcutaneous method. The first is the operation as performed by M. Velpeau, and the other that of Dr Pancoast of Philadelphia.

M. Velpeau was evidently dissatisfied with all the operations that had been performed for the radical cure of reducible inguinal hernia; but he was unwilling to believe that no remedy could be found for it. The success which so often followed the operation for hydrocele by injection, led him to think that a similar course might produce the same results in the treatment of reducible hernia.

He accordingly performed the operation on the first favourable case that presented. An incision of an inch in length was made just below the external ring down to the neck of the sac; this was opened with a bistoury, and a mixture of six drachms of tincture of iodine in three ounces of water was thrown in. An assistant compressed the inguinal canal, so as to prevent the fluid from coming in contact with the peritoneum above the ring. After the injection had been pushed around the various parts of the sac, it was allowed to escape through the canula. No unpleasant symptoms followed; but the final result of the experiment has not, as far as we know, been made public.

M. Velpeau does not seem to have much confidence in the operation, and it is understood that he does not continue to perform it at the present day. He has probably learned that something more than the mere closure by the process of adhesion of the neck of the sac is necessary for the radical cure of hernia. The fibrin that is effused will in most cases be soon absorbed, so that the barrier which had been relied on to prevent the descent of the hernia will be entirely removed.

About the same time, Dr Pancoast performed the operation which is described in his work on "Operative Surgery." The hernial sac, its contents having been previously returned, was punctured with a small trocar passed through a canula. Having ascertained that the instrument was fairly in the sac, by the freedom with which it could be moved about, the point of it was then directed upwards so as to scarify the internal surface of the upper part of the sac. The trocar was then withdrawn, and half a drachm of the tincture of iodine, or an equal quantity of the tincture of cantharides, was thrown in slowly by means of a small syringe fitted to the canula. The canula was then withdrawn, and a compress was applied just above the external ring, and the pad of the truss, which had been put on before the operation, was brought down over the compress.

This operation was performed in thirteen cases, in one of which only were there any symptoms of serious inflammation, and these readily yielded to leeches and fomentations. On some of these patients a single operation was performed, and on others, in whom the sac was large, several were required. All of them were evidently benefited at the time, but whether a radical cure was effected in any instance could not be ascertained, as nothing was known of the patients after a few months from the time of the operation. Whether Dr Pancoast continues to practise it, we are unable to say.

This method has, in the opinion of the Committee, all the advantages of that of M. Velpeau, while it avoids in a great degree the danger of peritoneal inflammation, to which patients are exposed by his mode. When the hernial sac is laid open, there is of course a direct communication between the abdominal cavity and the external wound. This alone would be likely to excite inflammation, and if, in addition, a part of the peritoneum is subjected to the action of an irritating fluid, there is reason to fear that the inflammatory process would not be limited to the sac, but that fatal peritonitis would be the consequence.

Admitting that these operations accomplished all that they were designed to do, it does not follow, by any means, that they would in every instance produce a radical cure. All that they could effect, if successful, would be to close the neck of the sac, without contracting the tendinous opening or ring. Sir Astley Cooper very truly says, "that, although the original sac may be completely shut at its mouth by adhesion or perfect contraction, it is possible that another sac may be formed contiguous to the first." In fact, it is well known that sometimes the hernia has recurred, after the whole of the original sac has been removed. Contracting or even closing the neck of the sac is evidently then not enough; "something more," says Mr Lawrence, "is required; we want a remedy that should contract the tendinous opening; for while that remains preternaturally large, a new protrusion is a highly probable occurrence."

This has been attempted in two ways. The first is by scarification of the external ring in inguinal hernia, and the other is by means of sutures. The first of these is quite an old operation. Heister says that "some surgeons scarify the ring of the abdomen, or aperture through which the intestine prolapsed, together with the skin, in order to render the cicatrix more firm; by which means many have been cured of these ruptures, especially if they continue to wear a proper bandage for a considerable time afterwards. But I think that the operation may succeed better in infants than in adults."

It is perhaps enough to say, with regard to this method, that it has been occasionally tried from time to time, for more than a hundred years, without a sufficient degree of success to gain the confidence of surgeons; and it is not to be overlooked that the danger of wounding the epigastric artery is no inconsiderable one; enough at any rate to deter all but the most expert from attempting to perform it.

The operation of closing the external ring by means of sutures is, we believe, quite a recent one. It is proposed by Thomas Wood, M.D., of Cincinnati, who states that he has performed it in three cases with success. His paper on the subject may be found in the last volume of the *Transactions* of this Association. It is certainly entitled to great consideration, not only from the importance of the subject, but from the candid manner in which he has treated it.

He says that his "experience is too limited to warrant him in saying much in its favour, but he cannot refrain from expressing the opinion that it offers to the ruptured patient a better prospect of a 'radical cure' than any operation before proposed."

He thinks that all the preceding operations have failed because, from the nature of the texture concerned in them, the adhesions have not been sufficiently strong to prevent the descent of the hernia; and he founds his expectations of success from his method on the following considerations: "Tendons," he says, "when wounded, will unite again by a formation similar to their original structure."

"Tendon is a permanent, unyielding tissue, seldom ruptured by the strongest exertions of the body."

"If we can close the external ring by a tendinous growth, we may effect a 'radical cure of hernia.'"

We do not deem it necessary to go into the details of the operation; for these, we refer to the author's paper. But we would remark, that it is by no means certain that tendons, when wounded, are united by a similar substance. Much light has been thrown on this point within the last few years by the numerous cases in which tenotomy has been performed; and the Committee think that they are justified in saying, that it has been ascertained that the divided edges of tendons are united by a substance less resisting, more elastic, and not so firm as the original texture of the tendon.

But, although this operation may not be found on further trial to be more successful than that of the scarification of the ring, yet, as it proposes to accomplish what has never been effected in any other way, viz., the contraction of the tendinous opening, it certainly, on this account, if no other, merits the careful consideration of the profession.

An operation similar to that of Dr Pancoast, if not precisely the same, has been performed to a considerable extent in the neighbourhood in which your Committee reside. Many persons, it is said, have been cured by it; but we have not met with any one of them who has felt that the truss could be safely laid aside. In one instance which has come to our knowledge, an individual submitted to the operation and thought himself cured. In a few months after, he gave up his truss, supposing that compression was no longer required, but in eighteen months from that time the hernia returned.

It is not pretended, however, that a cure may never be effected by this method, when all the circumstances of the case are favourable. It may happen, sometimes, when the hernia is small and recent, and when the patient is in good health and young, or has not passed the middle age of life; and it may too be of great advantage in some cases in which the hernia could not be kept up by the truss alone, as this operation would be likely to cause an abundant effusion of fibrin in and about the neck of the sac.

Two of your Committee have had some personal knowledge of it; the results of their observation and experience will be found at the end of this report.

It is an unquestioned fact that reducible hernia is often cured in young subjects. It may be accomplished in them by various means; but it should not be thence inferred that the same course would uniformly produce like effects in adults.

It may be remarked that, in children, any method which can prevent the protrusion of the hernia for a year or more will, in all probability, produce a permanent cure. If the aperture through which the contents of the sac must pass can in any way be prevented from enlarging while the viscera of the abdomen are increasing in size, it is obvious that a great length of time would not be required to render an escape of any of the abdominal organs difficult, if not altogether impracticable. We see familiar examples of this daily in umbilical hernia, which is brought on so often in infancy by hooping-cough and various other causes. *Compression*, it is well known, will, in all such cases, if carefully practised, in a comparatively short period produce a radical cure; and it is a valuable agent in the management of reducible hernia at every period of life. It has been used from the time of Celsus to the present, and it has not unfrequently succeeded in producing the desired result. It is usually applied at the present day by means of trusses. Great improvement has of late years been made in their construction. It cannot be doubted that an instrument of this kind, when nicely adjusted, so as to cause no pain or inconvenience, and at the same time to compress the neck of the sac, may, if used for a considerable length of time, prevent in many cases the subsequent protrusion of the hernia.

It is well known that pressure upon a serous membrane, when carried to a certain extent, will cause an effusion of fibrin on its inner surface, and it was from a knowledge of this fact that, in former times, the method of treating aneurism by compression was adopted. This mode often succeeded in producing a radical cure, by closing the artery leading to the aneurismal sac. The practice has been revived with great confidence within the last few years, and the results hitherto have been equal to the expectations of its advocates.[1]

In the treatment of hernia in this way, it is of the utmost importance that protrusion should not be allowed to take place at any time; "for if the hernia once descends during the wearing of the truss," as Sir Astley Cooper well remarks, "the cure must be considered as recommencing from that moment." The truss, therefore, should be worn by night as well as by day.

It is important, also, that while the pressure is sufficient to prevent the descent of any of the abdominal contents, it should not be enough to cause any considerable degree of inflammation. This would not only require the truss to be laid aside altogether, but it would also stop entirely the effusion of fibrin. In inguinal hernia, the pad should be so placed as to compress the inguinal canal; and at the same time great care should be taken to avoid pressing the spermatic cord against the pubis.

A radical cure will not be effected in this way, unless the compression is continued for a length of time. It cannot be reasonably looked for in an adult in less than two years from the time the truss is first worn; and it can hardly be expected at all in persons after the middle age of life, who are afflicted with a direct inguinal hernia of long standing. At the same time, more benefit is derived from compression in such cases than from anything else, and persons in this situation are not safe without it.

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[1] See Edin. Med. and Surg. Jour., volume seventy-sixth, July 1851, p. 211.

The Committee beg leave to offer the following opinions as the result at which they have arrived after a careful examination of the subject committed to them.

I. That there is no surgical operation at present known, which can be relied on with confidence, to produce in all instances, or even in a large proportion of cases, a radical cure of reducible hernia.

III. That they regard the operation of injection by the subcutaneous method as the safest and best. This will probably in some cases produce a permanent cure, and in many others will afford great relief.

III. That compression, when properly employed, is, in the present state of our knowledge, the most likely means of effecting a radical cure in the greatest number of cases.

All of which is respectfully submitted, by

GEO. HAYWARD,
J. MASON WARREN, } Committee.
S. PARKMAN,

Appendix to the Report on the Permanent Cure of Reducible Hernia.

No. 1.—*Statement of J. Mason Warren, M.D.*

In October 1847, some operations were performed by me, at the Massachusetts General Hospital, for the radical cure of hernia. They were, I believe, the first done at this institution with this object, and have since been repeated there by myself and others.

The method proposed by Dr Pancoast some years since, modified a little according to circumstances, was pursued in some of the cases. It may be described as follows:—

The contents of the hernial sac being returned into the abdomen, and the ring explored to ascertain that no portion of the intestine protrudes, the pad of a well-fitting truss is slipped down so as to make pressure on the inguinal canal, and prevent any escape of the hernia. With the forefinger of the left hand the spermatic cord, as it passes out from the external inguinal opening, is pressed upwards on the pelvic bone, so as to prevent it from being injured. A delicate trocar and canula, the latter having fitted to it a small Anel's syringe, is now carefully but firmly forced through the integuments with a rotatory motion to facilitate its progress, and pushed forwards till it enters the external inguinal ring or neck of the sac. The trocar being now withdrawn, the canula is kept firmly in place, and twenty or thirty drops of the tincture of iodine, tincture of cantharides, or sulphuric ether, thrown in and lodged in the neck of the sac, when this is practicable, or else in the vicinity of the external abdominal ring. Subsequently to the operation, a small compress is applied over the minute wound made by the trocar, the pad of the truss slipped down over it, and the patient directed, for a week or two, to maintain the recumbent position.

In addition to the injection, in some of the operations, a tenotomy knife was previously introduced, and the internal surface of the neck of the sac scarified. The wound made by the knife in these cases much facilitated the subsequent introduction of the trocar, which is with some difficulty worked through the integuments.

In no instance did any bad result follow the operation—the pain and inconvenience hardly amounting to that presented in a case of hydrocele treated by injection, or in any simple operation.

The following case, attended with success, will serve as an illustration of the course generally pursued:—

A male child, three years of age, with congenital inguinal hernia of the right side, was brought to the Massachusetts General Hospital to obtain relief, if it was possible, as no truss had been found to retain the protruded intestine in the abdomen, and the pain and inconvenience from the infirmity were great. On examination, a tumour, the size of a small orange, was found to occupy the scrotum. By a little manipulation, the contents were ascertained to be a portion of omentum, a loop of intestine, and the testicle—the whole of which, by care, could be easily returned into the abdomen.

The question which first presented itself was, whether the testicle could be separated from the other parts, the adhesions being quite intimate between them, so as to admit the return of the intestine and omentum into the abdomen, leaving the testicle in the scrotum. This being determined, the operation was performed as follows:—The intestine and omentum being returned into the abdomen, and the testicle prevented from following, the spermatic cord was held out of the way in the manner stated above. A subcutaneous incision was then made by a cataract knife, the point of which was carried into the sac, and the neck scarified in different directions. Through the aperture thus made, a small trocar and canula were introduced; the former being withdrawn, the syringe was adapted, and thirty drops of sulphuric ether were injected. The truss was now applied.

The operation was performed in October 28, 1847, and there was every prospect of success, when, from a violent paroxysm of crying, the hernia was forced down on December 9. On the 12th, the injection was repeated, and resulted, on the following day, in a swelling of the scrotum, such as is observed after the injection for hydrocele. On December 22, the report was made that the hernia came slightly down, and was returned with difficulty, "the aperture being apparently quite small." By the end of the month, it was stated that the hernia was perfectly retained.

I have been informed by an individual who saw the patient a year afterwards, that the cure was permanent.

During the treatment of this case, a slight superficial suppuration took place under the pad of the truss, which, the patient being somewhat fractious, was necessarily applied pretty firmly to prevent the recurrence of the hernia after the operation.

In a large proportion of the other cases operated on, the patient experienced much relief, though still obliged to wear a truss. In one case, where the hernia was quite large, no relief was experienced. A female, with a double femoral hernia, on whom the scarification and injection were once or twice repeated, expressed herself much benefited by the operation, the hernia being retained, and the suffering previously experienced much relieved. Another patient, a labouring man, was seen by me six months after the operation; the rupture had not recurred, but he still wore a truss. Previous to this time, he had been unable to work without forcing down the intestine under the pad, causing so much pain and ill health as to have induced him to come to Boston for relief.

Some allowance for the want of perfect success in the above operations must, of course, be made from the tentative mode in which they were performed through fear of a serious result.

From a comparatively limited experience, I derive the following conclusions.

1. That the operation, when carefully performed, is safe.
2. That, in ruptures where the neck of the sac is small and the abdominal aperture not too much enlarged by repeated descents of the hernia, there is a prospect of a radical cure.
3. That, in most cases, the operation mitigates the infirmity, allowing the hernia to be more readily retained by the ordinary mechanical means.

J. MASON WARREN.

BOSTON, *April* 1852.

No. 2.—Statement of Samuel Parkman, M.D.

I have operated, at the Massachusetts General Hospital, in four cases for the permanent cure of hernia.

The first case was in January 1848; the second and third in September 1849; and the fourth in October 1851. The herniæ were all inguinal. The method employed was scarification of the pillars of the ring and adjacent parts, with injection of tincture of iodine, by means of a syringe armed with a point at the nozzle. The precise situation where the fluid was thrown I believe it to be impossible to determine. It is clearly more probable that it should have been into the cellular tissue around the neck of the sac, than into the sac itself. In all the cases I compressed the canal to prevent communication with the peritoneal cavity. Rest in the horizontal position was enjoined, and a bandage applied. Some tenderness and swelling resulted in all the cases.

The first patient considered himself relieved, that is, he felt that the hernia descended with more difficulty.

The second and third cases presented no marked results.

The fourth case is worthy of note.

A healthy man, twenty-five years of age, stated that the right testicle did not descend until his tenth year, and was then followed by a hernia. This hernia had always been reducible, but the testicle was returned with it. A truss had but imperfectly retained the hernia, and he was desirous of relief if possible. On examination the ring appeared very large, and coughing or straining produced the descent of a mass the size of a goose-egg, on the front of which was the testicle, which could not be separated from the hernia proper. I proposed to retain the whole mass, and with this view, between the 29th October and 10th November, I did four separate operations, each time scarifying the pillars of the ring and injecting tincture of iodine. Each operation was followed by an effusion into the parts, and on November 14, when the patient was discharged by his own request, he stated that he had a feeling of firmness in the ring which he had not previously. I have no doubt, however, that the hernia could readily have been made to descend.

The conclusion from this small experience appears to me to be,—

That the operation by scarification and injection, being perfectly safe, is worthy of being practised to test the amount of its efficacy; but the fact that so many other methods, which, *à priori*, would seem more calculated to produce the same expected result, viz., the plugging the neck of the sac or ring with lymph, have not cured the disease, should suggest very great caution in the formation of an affirmative opinion from any but a large and long experience.

The operation, as practised by Dr Wood of Cincinnati, also in my opinion offers many inducements to surgeons, to make it the subject of experiment; since, proposing, as it does, the *approximation* as well as agglutination of the pillars of the ring, it attempts to meet the objection that may be made against all the other methods, viz., that the tendinous opening remaining the same, but an imperfect barrier can be opposed to the redescent of the hernia.

S. PARKMAN.

BOSTON, *April* 9, 1852.

No. 3.—*Letter from Henry J. Bigelow, M.D., Professor of Surgery in the Medical School of Harvard College.*

BOSTON, *April* 15, 1852.

DR HAYWARD AND GENTLEMEN OF THE COMMITTEE—

GENTLEMEN—I have the honour to acknowledge the receipt of your circular respecting the radical cure of hernia. In reply, I would make the following statements of the result to which I was led by some investigation of the subject a few years since.

1. I know no method which affords much more than a chance of radically curing this affection.

2. There is a chance of curing hernia, and also a chance of permanently relieving it, by treatment other than the application of a common truss.

3. There is, on the other hand, a considerable chance that the patient will receive no benefit whatever from such treatment.

4. Some of the proposed methods of treatment are hazardous to the patient, and therefore inadmissible. But, on the other hand, some are comparatively innocuous; such as that by the hard truss, and also an operation which has attracted some notice in the neighbourhood within a few years, viz., that by scarification and stimulating injection, practised by Velpeau, and subcutaneously by Pancoast.

5. If a patient desires to undergo such a comparatively slight operation, for some chance of subsequent improvement, and for a small additional chance of permanent cure, he may be gratified by an operation of this last description. By improvement, is here meant such, for example, as an acquired ability to ride on horseback; or a greater facility in

retaining the hernia with a truss. There is little probability that the patient will dispense with his truss; and the hernial contents, if confined to the abdomen for a time, will be likely to escape again in the course of weeks or months.

6. It is well known that a small or recent hernia, or one occurring in the young subject, is occasionally cured by a truss, or by a horizontal posture; the abdominal contents being retained, while the tendinous aperture grows spontaneously smaller. But such spontaneous contraction of the ring does not often take place, and surgeons have devised many ways of plugging the ring with skin and other materials, by means of plastic lymph. This adhesive lymph has been the great agent relied upon to effect the cure.

7. The method by subcutaneous injection and scarification is probably the readiest and most harmless way of producing an effusion of this lymph in or about the neck of the sac. But lymph is essentially plastic, and yields to continued force. A wound of the abdominal parietes, thus united, is followed by hernia in the cicatrix; and a natural interval in the parietes plugged by lymph must have the same tendencies. Many patients have received no ultimate benefit from this operation. It may be also a question, how far a partial obliteration of the tendinous ring of a hernia may increase the liability to strangulation.

8. On the other hand, the contents of certain hernial sacs can be unquestionably thus glued *for a time* into the abdomen, and hindered from descending; and there are patients who, though they still wear a truss, believe their condition to have been ameliorated by an operation of this sort.

9. Having my attention called to the subject some years since, I have operated in a number of cases with various success; subcutaneously injecting the neighbourhood of the neck of the sac with tincture of iodine, taking care also to disturb and to lacerate the tissue with the point of the instrument, for the purpose of promoting the effusion of lymph. For details connected with one or more of these cases—for a description of the operation nearly identical with that practised by Pancoast—and for some brief remarks upon its merits and its contingent accidents, I take the liberty to refer the Committee to a clinical lecture delivered before the class of the Massachusetts Medical College, and printed in the *Boston Med. and Surg. Journal* in Nov. 1850.—I have the honour to be, gentlemen, with great respect, your obedient servant, HENRY J. BIGELOW.

This letter was not received until after the report was finished.

ART. V.—*On the present state of knowledge acquired in Hæmatology, and the Practical Consequences which thence result.* Thesis maintained by J. D. THOLOZAN, Accessory Professor at the Imperial School of Military Medicine and Pharmacy. Paris, 1853.

De l'Etat Actual des Connaissances Acquises en Hæmatologie et des Consequences quien decoulent. These Presentée et Soutenne par J. D. THOLOZAN, Professeur agregée a l'Ecole Imperiale de Medicine et de Pharmacie Militaires. A Paris, 1853, 4to. (FACULTE DE MEDICINE DE PARIS.)

"It is a mysterious fluid the blood. Nothing can with propriety be taken for granted respecting it. Its properties can only be ascertained by exact experiment and unbiassed observation."—JOHN DAVY.

PRELIMINARY REFLECTIONS.

The state of information acquired at any given period upon any subject of scientific research, consists not in all instances, as it may be believed, in a simple exposition of facts ascertained by observations and conclusions logically deduced from these.

In the study of the positive sciences, and particularly in that of the Natural Sciences and Medicine, there is no period at which the state of knowledge on any given subject is established with a sufficient degree of precision, and in a manner sufficiently certain, to form the minds of all to one same opinion; and there is not ground, if we desire to present a delineation and exposition of the actual state of science, for discussing different opinions, for contrasting opposite doctrines, for comparing with each other contradictory facts and diverging results, and for forming at the same time an estimation, and a critical judgment for the purposing of taking an inventory of our information.

In this truth I presume every one will agree. When the question relates only to different observations upon multiplied subjects, the duty of the historian may be confined to the bare enumeration of facts. But when, upon the same subject, the experience of observers of all periods, and particularly in our own time, has collected so many documents upon hæmatology as now require to be consulted;—when these documents treat upon very different points, yet all pursuing the same practical object;—when upon the same point they adduce contradictory data;—the duty of the writer is completely changed. He can no longer confine his efforts to follow, in this confused labyrinth, an infinite number of experiments, and to record results which are often void of exactness. To him belongs then a different study. It is that of ascertaining the value belonging to each result, the degree of precision and of weight of each series of experiments. It is no longer sufficient to make a rough inventory. It is requisite to investigate, to classify, to arrange the facts, in order to make manifest their object, their consequences, their deficiencies.

This manner of procedure is recommended by another consideration. This Dissertation treats not only upon the theoretical study of Hæmatology, but upon the practical consequences to be deduced from this study. It is therefore not only in relation to the scientific facts that we have to review hæmatological researches; it is further in reference to the practical applications which they are required to furnish. The practical consequences, however, of Hæmatology, embracing the indications which this branch of physiological knowledge may furnish for the employment of any given medicine, for the adoption of any particular method of medical treatment, are also, justly derived from the elements which Hæmatology furnishes to Etiology, to Diagnosis, and to the Prognosis of Diseases. There is, therefore, in this subject, which is of great extent, one side, so to speak, concealed, and which is only discovered by sounding the whole depth of the inquiry. Moderate reflection, indeed, shews that the practical consequences of a knowledge of Hæmatology possess a high degree of importance, and can be explained only by the aid of the rational interpretation of the doctrines taught on the part performed by the alterations in the blood in different diseases. It may further be seen, that the physicians who have made most progress in the knowledge of Hæmatology, who have in a great degree created this branch of knowledge, are those who have derived from it the smallest amount of practical applications, for the reason that they have been unwilling to depart from the immediate and strict interpretation of facts.

It would be difficult to find, in this respect, a better example of scientific caution, than that which has been given by MM. Andral and Gavarret. These inquirers have had no other object than to lay the foundation of the scientific part of pathological Hæmatology; and by a very bold application of synthetical reasoning, they have at the same time established upon these researches a classification of diseases, which enjoys great reputation.

These discoveries have encountered the fate which awaits the publication of all important discoveries. Scarcely had they appeared, when a great number of observers, taking the same line of inquiry, found in it some new facts, or at most obtained some different results of analysis. Under this head we shall speak of their researches. MM. Andral and Gavarret, in making a large step in science, never entertained the idea that it was to stop at the limits of their labours; nor did they establish for their contemporaries an exclusive field of research. I may even express the opinion, that if these physiologists were at present to make new researches, they would direct their attention to points different from those which formed the subject of these early inquiries. Many physicians seem in this matter to have forgotten that experimental investigation is useless when its object is not the acquisition of some new truth, or the confirmation of truths already acquired; and that if to discover, it is requisite most frequently to invent means of research; so also in order to confirm acquired results, it is necessary to possess greater practice in manipulation, means of inquiry more accurate, and greater authority than those possessed by the first observers.

These considerations will explain in some degree the manner in which I have composed this Dissertation, the different materials which I have employed, and the order which I thought it my duty to adopt. When I had resolved to treat of the information acquired in Hæmatology, I should have deemed my performance incomplete, had I considered the blood only as to its artificial separation, the proportion of Fibrin, of Albumen, of Globules, of organic and inorganic solid materials and of water. It appears to me that if MM. Andral and Gavarret were in the right, when they bestowed a smaller amount of attention on the different aspects and on the degrees of the inflammatory crust in a quantitative estimation of the Fibrin, a considerable number of questions were nevertheless still left for exposition, a considerable number of facts acquired, and of curious results appreciated, of which an analysis and estimation could be formed only by the aid of the profound study of the properties and phenomena of the blood. This investigation it appeared to me that I could make by the aid of the labours of Newson, of John Davy, of Gulliver, of M. Andral himself, and of Polli. Here however an obstacle presented itself. The labours of Hewson, of Davy, of Gulliver, are works purely physiological.

In them Pathology and Practical Medicine occupy little place. It was then my duty to avoid the rock which presented itself at the outset of this undertaking, in the large number and extent of the researches of physiologists upon the blood. It was my duty to make a selection in these researches ; and when called on to decide, I believed that I was performing the part of a Pathologist, if I confined myself almost exclusively to the study of the coagulation of the blood. This phenomenon is, of all those which the blood in the normal state presents, the most conspicuous, the most varied, the most complex, and that which suggests to Pathology the most important considerations, and the most practical applications.

Let it be remarked as to other points, that the phenomenon of the coagulation of the blood allies itself with all the theoretical and practical questions which the study of that fluid suggests. If we speak of the distinction of diseases into inflammatory and non-inflammatory, we have recourse to a proportionate amount of fibrin ; but how do we produce this amount of fibrin? By beating or agitating the blood. And is beating anything but a coagulation produced artificially? Further, when we separate the fibrin by washing the clot, we employ for the extraction of this principle the natural phenomenon of spontaneous coagulation. The importance then of understanding this phenomenon in all its details is manifest.

A volume might be written upon the mistakes of observers who have neglected the study of the coagulation of the fibrin. Some, observing that in certain circumstances the first blood letting was never buffy, while subsequent bloodlettings were progressively more so, have denied that the presence of buff upon the clot was a proof of inflammation. Others, observing the sizy crust upon the clot of anæmic and chlorotic persons, have maintained that there is in these cases an *angeioitis*, or a slow *arteritis*. Some physicians have wished to draw blood in all circumstances, in consequence of the presence of the buffy coat, the formation of which they conceived not to be possible without the presence of inflammation. All these mistakes arise from imperfect knowledge of the phenomenon of coagulation of the blood, and from the idea,—admitted at the present time by a great many physicians,—that some relation of causality subsists between the process of inflammation and the increase of the fibrin. The blind and entirely hypothetical reasonings, which have been made upon this subject, would by no means advance us in the study of the question.

In the year 1841 M. Andral made the following statement upon this point.

"I possess as yet no fact which shews the fibrin to be increased in quantity previous to the manifestation of the local action or disorder which, in any given solid, is allied with the inflammatory condition; on the contrary, in several instances as soon as I had occasion to examine alternately the blood of any individual on the evening of the day on which an inflammatory disease was about to attack him, and the day even on which the *phlegmasia* began, we were able to satisfy ourselves,—M. Gavarret and I,—that in the blood of the bloodletting drawn before the perceptible commencement of the *phlegmasia*, the fibrin of the blood was not augmented."

In no passage of the writings of this physician, in short, do we find the idea of making the augmentation of the fibrin nothing else than a means of distinguishing between the pure *Phlegmasiæ* or Inflammations, and the Idiopathic Febrile Disorders.

We know not how in the *Phlegmasiæ* this excess of fibrin is produced any more than we know how an excess of fibrin can give rise to *Phlegmasiæ*. We live amidst hypotheses on this subject; and in hypotheses there is great danger; the greatest of all being that many minds proceed upon them as upon facts acquired and demonstrated.

The principles now enumerated will explain and account for the tendencies of the first part of this Essay.

CHAPTER FIRST.

COAGULATION OF THE BLOOD.

Among all the phenomena pertaining to the knowledge of the constitution of the blood, none is more entitled to be studied than that of Coagulation. It is a natural change, which takes place of itself, without the intervention of external circumstances. It is observed in certain diseases during life; it is observed after death in the bloodvessels; the rapidity and the uniformity with which this process takes place in the blood, when drawn from the vessels by bloodletting, constitutes an important subject of inquiry to the physiologist.

I shall consider here, 1*st*, The coagulation of the blood drawn from the vessels during life; 2*d*, The coagulation of the blood within the vessels after death; 3*d*, The coagulation of the blood within the vessels during life.

ARTICLE FIRST.

THE COAGULATION OF THE BLOOD DRAWN FROM THE VESSELS DURING LIFE.

§ I.

Previous to studying the intimate mechanism of this Coagulation, I think it necessary to advert shortly to certain circumstances which exercise great influence upon this process. On this subject no inquirer has come near John Davy, for the accuracy and the bearing of his experiments. For complete information upon this question, I refer to his Observations on the Coagulation of the Blood, contained in the thirtieth volume of the *Edinburgh Medical and Surgical Journal*, October 1828, from which I give here the principal facts.

1. *Effect of Violent Agitation upon the Blood.*

Dr Bostock, in the first volume of his *Elementary System of Physiology*, remarks, that "It is well known that if the blood as it is discharged from the vessel be for some time briskly stirred about, the process of coagulation is entirely prevented from taking place, either in consequence of a more complete union of the different parts of the blood with each other, or from the fibrin, after it has been for some time discharged from the blood, losing the peculiar property which its particles possess of mutually attracting each other.

This statement, which, as is seen, is not new,[1] suggested to Davy the idea of repeating the experiment; and as it relates to a point of doctrine upon which a singular interpretation has been placed in our times, I shall re-state it in full.

"About two ounces of blood were received into a large vial, and immediately shaken violently, and the agitation was continued without intermission for ten minutes, which was two minutes after the blood at rest had coagulated. The result was that the blood thus shaken acquired a scarlet hue, from being mixed with, and from the action of, air, and appeared to be liquid. But this was merely in appearance. For when poured out on a filter it separated into two parts; one serum, with colouring matter, which passed through the filter; the other fibrin in a finely-divided state, coloured by adhering red particles, which remained in the filter."[2]

[[1] The statement had been made by all writers on animal chemistry and physiology many years before the publication of Bostock's treatise. It is made by Dr Thomas Thomson so long ago as 1812, and by others before that time. Dr Hope was in the constant habit of mentioning it in his lectures.]

[2] Edinburgh Medical and Surgical Journal, volume thirtieth, p. 248.

John Davy then compares the effect of agitation in these cases to that of breaking up the crassamentum between the fingers, when an apparent solid is converted into an apparent liquid. Nor is this at all surprising, he adds, considering the small quantity of solid matter contained in the crassamentum, to which it performs the part, as it were, of a cement, or of calcareous matter to sand.

2. *Effect of moderate Agitation upon the Blood.*

It seems easy at first sight to determine if moderate agitation does or does not accelerate the coagulation of the blood. Nevertheless, according to the learned experimentalist now quoted, considerable difficulties stand in the way to forming a just conclusion. The great difficulty in these researches, and in all inquiries relating to Hæmatology, is the impossibility of finding any two portions of this fluid positively and exactly similar to each other. In the opinion of Dr John Davy, no two portions of blood, and even no two drops, are exactly similarly constituted, or possessed of exactly the same properties. Numerous experiments place beyond doubt this fact, which had been previously rendered highly probable by reasoning.

" Six gallipots of the same size," says Dr Davy, " and a platina crucible of nearly the same size, were filled in succession with blood as it flowed from the arm. They were placed close to each other, and carefully observed. The blood in each did not coagulate, as one would have expected, in the order in which it was received, but irregularly. Thus the first received was the last but one to coagulate, and that received in the crucible, which was the fourth in succession, coagulated first."

" When one reflects on the subject, the inference appears to me to be obviously what I have suggested above,—that no two drops of blood are precisely similar in their composition and properties. It is evident on reflection, that the fluid of a continuous stream of blood must be derived from different parts of the body. In one portion there may be an unusual or less than usual quantity derived from the vessels of the muscles, or brain, or liver, or spleen, &c. ; and we cannot but suppose that the blood from the kidney, after urine has been secreted from it, is different from the blood from the liver or pancreas, after supplying bile and the pancreatic fluid."

"The preceding experiment and this reasoning are confirmed by another fact which I have observed, viz., that if the crassamentum, the instant it is formed, is removed, it will be succeeded by another coagulum; and if this is removed, by another; and so on repeatedly, till all the fibrin has coagulated,—clearly demonstrating, that in any given quantity of blood there are particles of fibrin of various degrees of rapidity of coagulation."

The facts here specified being known, it is easy to understand the contradictory results which have been obtained at all periods from the experiment of agitating the blood, from the time of Scudamore to the present day. Dr Davy infers that generally agitation of the blood favours its coagulation. When a small portion of crassamentum is thrown into blood, its coagulation is distinctly accelerated. The same effect seems to take place as when a crystal is placed in a saturated saline solution ready to crystallize; and it occurred to Dr Davy to think, that agitation may promote coagulation by producing a more intimate mixture of the particles,—of those particles which are first coagulated with those which are still liquid.

3. *Effect of Change of Temperature upon the Coagulation of the Blood.*

That cold retards the coagulation of blood, is a fact upon which all experimentalists are agreed. Hewson and Davy, concurring upon this point, saw also that cold increases the viscidity of the blood. At the temperature of 32° F., Davy saw the blood remain for longer than one hour; at some degrees lower it coagulated, and had the appearance of a homogeneous mass. After having kept it thus frozen for half an hour, the temperature was raised, and the blood became liquid and coagulated naturally as usual.

We may understand, consequently, the importance of this property of the blood in acquiring viscidity by exposure to cold, and being able to be coagulated without being altered, in reference to the fact of certain parts of the body being frostbitten in cold seasons and climates, and to the theory of the application of cold in certain inflammatory diseases.

As to the influence of elevated temperatures, at 120 degrees of Fahrenheit, the blood becomes more liquid, and coagulation is accelerated; at 100° F., coagulation is rather retarded than accelerated; at 80° or 90°, coagulation is less rapid than at 120°, and more rapid than at 100°.[1] It appears very extraordinary that Dr Davy calls it a paradox, that heat above

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[[1] The author has erred greatly in stating the temperatures noticed by Dr Davy; for they correspond—34°, 23°, 12°, 18°—neither to rates in the centigrade scale nor that of Reaumur. The numbers in the text are stated from those originally given by Dr Davy. These mistakes come not with a good grace from one who speaks with the confidence of M. Tholozan.]

100^c should increase the liquidity of the blood and accelerate its coagulation; but it is much more so, to perceive that cold renders this fluid thick and viscid, and yet retards or prevents coagulation.

4. *The effect of Different Vessels upon the Blood.*

The conclusion to which we are led on this subject is by no means satisfactory. According to Dr Davy, it appears that wood and polished metals retard coagulation, and that glass and earthenware accelerate it. The form and size of the vessel also exercise some influence; but this it is more difficult to estimate than that of agitation.

5. *On the effect of the Removal of Atmospheric Pressure upon the Blood.*

According to an experiment by Sir Charles Scudamore, the blood coagulates more rapidly under the vacuum than in the open air, and becomes of a colour considerably darker.[1] Dr Davy tried the experiment repeatedly, and obtained invariably the opposite result; that is, the blood coagulated as soon under the exhausted receiver, as blood which, for the purpose of comparison, was left exposed to the atmosphere; and the rate of coagulation is much the same, certainly not quicker, rather slower.

6. *Influence of Oxygen and of Carbonic Acid Gas in the Blood.*

The experiments of Sir Humphry Davy and Dr John Davy shew, in opposition to the statements of Sir Charles Scudamore, that the coagulation of the blood is not accelerated in a perceptible manner by the presence of oxygen, nor retarded by that of carbonic acid gas; and this is the case even in instances in which these gases were agitated with the blood.

7. *The effect of Water, Milk, Bile, and Urine, upon the Blood; the effect of Substances which do not Coagulate the Serum; the effect of Substances which Coagulate Albumen.*

Water, apparently by diluting the blood, by removing the particles of fibrin to a greater distance from each other, by suddenly cooling the blood, retards coagulation; but not so long as was believed by Dr Crawford, who formed his opinion on the subject merely from the appearance of the blood.

Milk, urine, and bile also retard the coagulation of the blood, and in a greater degree than water. Urine is well adapted as a medium for displaying the phenomena of coagulation of the blood, for the reason that it retards but does not prevent the process, and because it does not dissolve the red particles.

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[1] Essay on the Blood, by Charles Scudamore, M.D. P. 22.

As to the effect of substances which do not coagulate serum, and the effect of substances which coagulate albumen, all may be embraced in the following conclusion. The action of re-agents upon the blood, or the fibrin of the blood, in reference to coagulation, is extremely variable, and cannot be anticipated *à priori*. It is not susceptible of explanation by any of the hypotheses hitherto advanced. Each of these effects demands special consideration, and minute experimental research, for its full elucidation.

§ II.

PHENOMENA RELATING TO THE PRODUCTION OF THE BUFFY COAT.

To Sir Everard Home, and his assistant M. Bauer, we must attribute, in modern times, the reproduction of the ancient hypothesis of Sydenham, namely, that the fibrin is formed, during the coagulation of the blood, of the colourless matter of the red globules. This hypothesis was so generally admitted in England, in France, and in Germany, that Müller appears to have rendered a great service to science in performing his ingenious experiment of mechanically separating, by filtration, the red globules on the one hand, and the serum and the fibrin on the other. Nevertheless, in 1771,[1] Hewson had demonstrated in a very precise manner the fact, that the coagulable matter of the blood is fluid after its issue from the vein. By means of neutral salts, which retard or prevent coagulation of the blood, he was able to obtain the fibrin in the Coagulable Lymph, as he calls it, detached from the globules. He had discovered that the red globules are precipitated more rapidly in the liquor of buffy blood than in isolated serum; and after this, it is not astonishing that he ascribed the formation of the buffy coat to the greater fluidity of the blood, and to the more rapid precipitation of the red globules towards the lower part of the clot.

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[1] We must not forget to say here, that Richard Davies, entirely forgotten for a long time, had, previous to Hunter and Hewson—namely, in 1760—published very correct facts, and some very judicious ideas, in his Essays on the Human Blood.

[A good account of the labours of Richard Davies is given by Mr Gulliver in his edition of the works of Hewson, Sydenham Society, published in 1846. See page xxxv.]

The employment of the microscope has thrown considerable light upon this process. It has thus been seen that the particular attraction which, in certain circumstances, the red globules exercise upon one another, performs an essential part in the formation of the buffy coat. John Hunter, speaking of the blood in inflammation, says, that the red globules are less uniformly disseminated in it, and that their attraction for each other becomes stronger, so that the blood when out of the vessels soon becomes cloudy or muddy, and dusky in its colour, and when spread over any surface presents a mottled appearance, the red particles forming distinct insulated spots.[1] But in making these observations Hunter did not avail himself of the aid of the microscope; at least everything tends to make us think so; and Nasse was the first person who shewed, that in buffy blood the red globules are accumulated side by side, and are separated in consequence of more complete separation of the fibrin.[2] This increase in the attraction of the globular discs for each other, must be regarded as the condition essential to the formation of the buffy crust; because this piling of the globules increases the specific gravity of each mass, and allows gravity then to act in a secondary manner.

Gulliver justly remarks, that these small masses of globules are sometimes visible by the naked eye. We know, he says, that in the same liquid very fine particles of a body will remain at the surface, while other particles more voluminous of the same body sink more rapidly than the former; and this observer explains in this manner the descent, at first very slow, that is at two minutes for one line, then more rapidly, namely, five or six times the first rate of velocity, in the course of the two or three following minutes, of the blood globule, in proportion as this is associated in sinking with agglomerations of globules more and more considerable. The demonstration of this fact further results from this circumstance, that these mixtures which destroy the aggregation of the globules without diminishing the density of the blood, increase the rapidity of the descent of the globules, while the mixtures which destroy the aggregation of the globules prevent or retard their descent considerably, though the blood is rendered more fluid and more light.

In order to complete this exposition, I cannot do better than give the following conclusions which flow from the observations of Gulliver, upon the formation of the buffy coat.

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[1] Hunter's Work by Palmer, volume first, p. 234, 235.

It is upon this phenomenon that Van der Kolk and Alison have wished to found a clinical method of distinguishing inflammatory blood from other sorts of blood.

[[2] This was fully explained by Mr Wharton Jones, in a paper published in this Journal, volume sixtieth, Edinburgh, 1843, p. 309, 310.]

1. There is a remarkable acceleration, after a few minutes, of the rate with which the red corpuscles sink in the *liquor sanguinis;* and in the serum alone, though to a less degree.
2. This acceleration may be increased by increasing the aggregation of the corpuscles; and prevented or reversed by preventing or destroying the aggregation of the corpuscles.
3. The sinking of the corpuscles may be slower in blood thinned by weak saline solutions than when mucilage is added with the salt.
4. The sinking of the corpuscles is slower in serum made thinner and lighter by weak saline solutions, than in serum made thicker and heavier by mucilage.
5. In the blood of the horse, the buffy coat forms regularly; and the red corpuscles unite, as if partly fused into each other, and collect into masses.
6. There may be no buffy coat, or but a comparatively thin one, on this blood, when it has been made thinner and its coagulation retarded.
7. The formation of the buffy coat is neither due to an attenuation of the *liquor sanguinis,* nor to a diminution of its specific weight, nor to slow coagulation; but to an increased aggregation and quickened sinking of the corpuscles.
8. These facts are favourable to the old doctrine of lentor or viscidity of the blood and union of the corpuscles; and against the more recent doctrine of attenuation of the blood in inflammation.
9. The corpuscles of the horse sink much quicker in his serum than the corpuscles of man do in his.
10. Increasing the proportion of the corpuscles hastens coagulation and diminishes the formation of the buffy coat more than increasing the serum only.
11. Increasing the proportion of water simply does not hasten the coagulation of the blood, as increasing the proportion of serum does.[1]

ARTICLE SECOND.

COAGULATION OF BLOOD WITHIN THE VESSELS AFTER DEATH.

The separation of the elements of the blood into solid and liquid parts rarely takes place, and is never completely accomplished, so long as this fluid is contained within the heart and bloodvessels, unless the structure of these parts is altered, as in instances of Dilatations, Aneurisms, and similar lesions. Ordinarily the blood is entirely liquid, or entirely taken in mass; sometimes we meet with a mixture of liquid blood and pale or blackish clots. It is to be regretted that the attention of anatomists has been little directed to this subject, which required to be completely created, when two observers of remarkable talent and accuracy, Dr John Davy and Mr James Paget, published, the former in 1839, the latter in 1841, twelve years ago, the results of their observations upon this phenomenon.

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[1] On the Buffy Coat of the Blood, by George Gulliver, F.R.S., in Edinburgh Medical and Surgical Journal, volume sixty-fourth, p. 374, October 1845.

Among 164 instances in which Dr Davy had noted the state of the blood within the vessels, he found it one hundred and five times coagulated, and containing fibrinous excretions; seventeen times coagulated and divided in fragments, as if by the contractions of the heart being continued feebly for some time after apparent death; fourteen times liquid; twelve times in the state of soft clot or purely grumous coagulum, and without fibrinous concretions; sometimes partly liquid, and partly coagulated; sometimes entirely, or almost entirely, wanting in the heart; once only he found fibrinous concretions without blood, that is destitute of liquid blood or of sanguinolent serum. In thirty-seven instances observed by the same physician at Chatham, the conditions of the blood were noted with minute precision; and in not one of these did he recognise limpid serum.

One case alone forms an exception. It was a case of Phthisis, in which a mass of fibrin found in the right ventricle contained a collection of transparent serum. This mass was firm externally. The right ventricle contained likewise fibrinous concretions, and a large quantity of cruor. Two hours after the dissection of the body, this cruor had become coagulated into a jelly. Twenty-four hours afterwards, the clot had become contracted, and the serum had become entirely separated.[1]

Mr Paget is still more explicit. " In all cases," he says, " it must be remembered that the coagulation that takes place in the body is much slower than that which ensues in blood drawn from it, either during life or after death; so that a quantity of uncoloured fibrin is found in the heart and uppermost vessels of the dead body, in many cases, in which it is most probable, that had the blood been drawn during life, it would not have presented a buffy coat. In the majority of cases the blood does not coagulate in the body for the first four hours after its rest has commenced; in many it remains fluid for six, eight, or more hours, and yet coagulates within a few minutes of its being let out of the vessels."[1]

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[1] Researches Physiological and Anatomical. Vol. ii., p. 190–203. London, 1839.

Six years after the publication of these remarks, the author states that he had never found clear serum in any part of the body after death, so long as the blood was contained within the vessels.

The announcement of these researches will probably be sufficient to shew how imperfect still is the doctrine of Hæmatology in all those questions which relate to the state of the blood after death, and to the phenomena which are produced in this liquid when it is allowed to rest in the parts in which it is naturally contained. To these data I subjoin an extract from the first Essay of Mr Paget upon Bloody Concretions of the Heart and of the bloodvessels in the London Medical Gazette for January 1841.

" Having, without any definite object, taken accurate notes of the condition of the blood found after death in the principal parts of the circulatory system, of nearly 150 of the bodies which I had lately examined, I was anxious to discover whether an analysis of the numerous facts thus collected would lead to any general results. Among several of more or less interest was the following :—Whenever in the coagula found after death in the heart and large bloodvessels the colouring matter has separated from the fibrin of the blood, so that a part of each coagulum is nearly colourless, and more or less similar to the buffy coat of blood drawn from a person labouring under an acute inflammation, the least coloured portion is always placed at that part of the clot which, in the position in which the body has lain since death, is at the highest level.

" In proof of this, if, in any case in which coagula are found in the heart, that organ be either examined as it lies in the chest, or be held in the position which it naturally occupies in the recumbent body, it will be constantly found that the uncoloured portion of each coagulum is placed uppermost, that is, in the part which in the body lies nearest to the sternum ; and that as the distance below the sternum increases, either the coagulum becomes more and more deeply coloured, and less firm, or else at some definite lower level presents an exact plane of separation between the uncoloured and coloured portions. A similar observation may be made in the long-branched coagula that lie in the pulmonary arteries. In the trunk of the artery these coagula are rarely quite black; they are usually either colourless, or contain but a few of the blood-globules, and these placed at the most depending part. But however little colouring matter there may be in the part of the coagulum which lies in the trunk of the pulmonary artery, the extremities of its numerous ramifications, which are contained in the posterior branches of that vessel, are always more or less completely black. The same rule is observed in the coagula in the branches of the pulmonary veins, and in those which occupy the vena cava superior, and its chief branches; in all of which the portion of the coagulum from which the blood-globules have separated is always that which lies on its uppermost (or, as the parts are usually described, its anterior) surface. Again, a similar fact is evident in the coagulum usually contained in the longitudinal sinus: if that canal, at the vertex of the head, contain a colourless coagulum, it will constantly be found, on drawing it gently out, that it is black at that part where it passes down the occiput; and whatever be the colour of the coagulum in the longitudinal sinus, the blood in the lateral sinuses is never, as far as I have seen, more than loosely coagulated in a soft black mass. In short, wherever the blood is examined, illustrations of the same rule are met with; as, for further examples, in the invariable blackness and softness of the coagula (if any have formed), in the vena azygos and vena semi-azygos; in the blood of the renal veins, as compared with that of the vena cava inferior adjacent to them; in that of the descending branches of the internal iliac, compared with the firm and often nearly colourless coagula in the external iliac and femoral veins; in the soft black coagulum of the thoracic aorta, which lies low in the posterior curve of the spine, compared with that which in the same subject is often colourless in the abdominal aorta on the anterior lumbar curve.

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[1] London Medical Gazette, January 1841, p. 618. Volume for 1840-41.

" It follows from these facts, which I have confirmed by the careful examination of a considerable number of subjects, that, regarding the cavities of the heart, and the large vessels leading to and from them, as one cavity, any quantity of blood contained in it at the instant of death coagulates according to the same method as it would if drawn from the body into a basin, or any other vessel; and that if its coagulation be sufficiently slow, or the proportion of fibrin large, the colouring particles always sink to the lower level, and leave a certain quantity of uncoloured fibrin above them. In different cases, this fibrin, as well as the rest of the coagulum, varies considerably in its general aspect, and in the degree of its adhesion to the walls of the heart; and according to the condition of the blood from which it is separated, presents nearly all the varieties which have been described as unorganized, and slightly or partially organized polypi, or concretions, of the heart.

"The rule applies equally whatever be the degree of coagulation of the blood. The best illustrations of it, however, are in the cases where the cavities of the heart are all filled with firm coagula, in which there has been almost a complete separation of the blood-globules from the fibrin, so that large white, yellow, or pinkish masses, of a firm, transparent, or glistening jelly-like substance, are found completely filling one or more of the cavities, and closely adhering to their walls, and in the interspaces of their muscular fasciculi. In these, which it is to be observed are the very cases upon which the opinion of the frequent coagulation of the blood during life is chiefly founded,[1] the vena cava inferior is full of soft black coagulum, without any trace of buffy coat. This is, on the one hand, continuous with a white or white and black coagulum, which fills a portion of the inferior cava lying on the lumbar vertebræ; and on the other, forms one mass with that in the right auricle, in which there is always a distinct level, above which all the coagulum is composed of more or less pure and firm fibrin, and below which it is quite black and much softer. The coagulum in the auricle is again continuous with that in the superior cava and its branches, in which there is usually the same degree of separation of the chief constituents of the blood as in the blood in the auricle, and always an equally distinct gravitation of the colouring particles to the lower or dorsal portion of the coagulum. Lastly, the coagulum in the right auricle is usually continued in one mass into that in the ventricle, where again similar characters may be remarked. The apex of the right ventricle being, in the recumbent posture of the body, the highest point of the heart, there the blood is never in these cases at all coloured; commonly the whole coagulum in this ventricle is white or yellow, but in many cases it exhibits a black layer at the most depending part; that is, at that part which lies in contact with the posterior wall of the ventricle, behind the auriculo-ventricular opening, and just below the origin of the pulmonary artery."

These facts from one end to the other afford confirmation of the data which have been stated above, upon the formation of the buffy crust and that of the clot. The slow but complete precipitation of the globules when the fibrin does not coagulate quickly; the more complete separation of this principle when the globules have sunk to a certain level before coagulation is effected, and proceeding after death in the different ramifications of the sanguiferous tree, the same hydrostatical phenomena as in ordinary vessels in the open air, are here remarked. I know not in Pathological Anatomy any experimental demonstration equally precise of a process so complicated as that of the Coagulation of the Blood, which, examined in the vessels after death, furnishes results perfectly agreeable to those which theory, deduced from observation of the process in ordinary vessels, could scarcely have ventured *a priori* to affirm.

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[1] They occur most frequently, but not exclusively, in those who die of acute inflammations. I have see such appearances in two persons who died of hæmorrhage, which was in neither preceded by any inflammation; and in the bodies of many who have died of perfectly chronic disease.

ARTICLE THIRD.

OF THE COAGULATION OF THE BLOOD IN THE CIRCULATING CHANNELS DURING LIFE.

Coagulation of the blood during life is a process as mysterious as that of ordinary coagulation. It is produced, it is modified, by multiplied influences; and if the blood appears here never to undergo coagulation without a previous alteration in the parietes of the canals within which it moves, it might be said that it coagulates spontaneously during circulation even and during accelerated circulation. In other circumstances, as physiologists have ingeniously done, if we inclose blood within a vessel for several hours, tying this tube at its two extremities, in such manner that inclosed fluid receives no circulatory impulse, we find at the end of the time the blood entirely liquid. If, in this instance, we evacuate the blood, it is coagulated immediately after coming out of the vessel.

L'Heritier applied two ligatures over the jugular vein of a Guinea Pig, at the distance of one inch and a half from each other; and after covering the wound he allowed the animal to rest for fifteen minutes. Upon opening the vessel, the blood sprung out liquid. Analogous experiments performed upon vessels tied at first, when full of blood, afterwards cut out, and completely separated from the animal, have always given rise to a speedy coagulation of the blood. The previous experiments of Hewson and of Thackerah confirm those which Heritier published in his Treatise upon Pathological Chemistry in 1842.

Along with the most obscure points in the history of the coagulation of the blood, may be placed the determination of the influences which indefinitely retard and adjourn this process, so long as the blood is in contact with living tissues, and this not only within the bloodvessels, but further in certain cases of extravasation. Thus, along with the experiment of Hewson, who after having inclosed in a ligature the jugular vein of a dog, found within the inclosed space at the end of two hours and a quarter only a small coagulum, we may cite those effusions of blood, the consequence of contusion, in which blood extravasated evidently at the moment of the accident is withdrawn a long time afterwards as liquid as if it had flowed from a vein, and is coagulated in a vessel in the course of some minutes.

Before entering upon the exposition of these phenomena, I may be permitted to return to the Pathological Anatomy of the dead blood, of the blood extracted from the vein, in order to inquire whether this blood thus deprived of life do not sometimes present phenomena similar to the anomalies which we have now met at the commencement of this study of living blood.

From a patient labouring under putrid fever, Dr Haen relates, two bloodlettings had been drawn. The blood of the second bloodletting, after a certain time was covered with a thin pellicle, under which was found a serous fluid, and beneath there was another membrane, so that the apparent serum was found inclosed as it were within a sac. Beneath this sac appeared the *cruor* under the aspect of a red powder. Having left this blood thus dissolved, it was found twenty-four hours afterwards entirely coagulated, and presenting a true inflammatory crust.

This fact, which De Haen finds surprising,[1] and the cause of which he acknowledges that he does not comprehend, remains unexplained at this day. Nowhere do we find a more complete description of it than in the observation which is recorded by Polli. This I give here without abridgment.

A patient suffering under severe pneumonia gave at the first bloodletting blood which did not coagulate before fifteen days. The first days after detraction, this blood presented at the surface a layer of clear fluid of a serous aspect, in which was lying a badly united gruel of red globules. On the eighth day the surface was covered with a fine membrane, in which was found contained as in a purse a considerable quantity of this serous fluid. Upon puncturing the membrane and collecting in a glass a part of this liquid, after some days, it was so completely coagulated, that it was possible to invert the glass without spilling a drop. That which floated upon this blood in the form of clear liquid, was therefore in great part only fibrin which was slow in coagulating.[1] In point of fact, fifteen days after extraction from the veins, this blood presented a consistent coagulum covered by a resisting buffy crust, which constituted at least the two upper thirds of the whole.

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[1] De Haen Ratio Medendi R. I., Cap. vi. 1757.

[This case is taken from Hewson; see Sydenham edition, 1849, p. 108. Hewson himself gives a similar case, which, however, is not mentioned by the present author.]

It is requisite at the present time therefore to be informed of this fact, and before pronouncing that in scurvy, putrid fevers, and similar diseases, the blood undergoes no coagulation, it is requisite to wait a sufficient time, to wait until the instant when putrefaction commences.

I have studied in all the difficult and complicated parts of this performance to obtain for my aid the light of Pathological Anatomy, and to ascend from the study of visible and tangible phenomena to that of the mysterious processes of Nature. I am unable to say to what extent I shall succeed in this path; but I shall at least have the firm conviction, that it is the only way to be pursued in the interpretation of the facts which science at present possesses upon the subject of the spontaneous coagulation of blood within the vessels.

We remark that the liquid state of the fibrin is a species of vital liquidity; that the cessation of this liquid state, coagulation, is a sort of cessation of this Life of the Blood. Numerous experiments[2] prove that this phenomenon is not owing to the agglomeration of solid particles, white globules, or other granulations contained in the blood. Further, when it is said that the fibrin is dissolved in the blood, we announce a fact of which there are no proofs; for we are unacquainted with any true solvent of fibrin; we know not whether in the living blood the fibrin requires a solvent. I propose in the meantime to apply to this slowly coagulating fibrin the name of *Bradyfibrin;* this changes nothing in the process, which remains surrounded by its original obscurity.

In the writings of Physiologists is found the record of ingenious observations which they have made, in order to determine whether the coagulation of the blood was not in some sense a species of crystallization. Observers have seen the buffy crust present at first a thin pellicle, radiating from the circumference to the centre, the fibrous prolongations of which become gradually thicker in proportion as the pulpy crust is developed. Observers have seen the coagulation of the fibrin accelerated by means of slight vibrations communicated to the vessel containing the blood, in the same manner as in a concentrated solution of a body about to be crystallized a slight agitation induces the formation of crystals. It has been seen that in the blood, as in the crystallization of saline solutions, the immersion of a solid body hastens the process of coagulation.

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[1] Annali di Medicina, vol. cix. p. 99. Milano, 1844.

[2] Anderson in Froriep's Notizen, August 1844.

We have spoken of the multiplied, and in some degree opposite, influences of rest and agitation upon the coagulation of the blood. The same result is produced during life. There are large varicose sacs, in which the blood remains sometimes entirely liquid, and in which, notwithstanding, it is in a state of complete stagnation. There are fevers in which, notwithstanding a febrile commotion most intense,—notwithstanding the incessant beating and agitation of the blood within the heart,—no coagulations are deposited upon the surface or edges of the valves, of the *columnæ carneæ*, of the *chordæ tendineæ*. In other circumstances, physiologists have been able to ascribe to the presence in the blood of an excess of fibrin, the existence of fibrinous concretions in the spleen, the kidneys, the liver, the heart, and the valves. These facts I merely mention, without explaining them, and without wishing to discuss them. The same is the case with those instances of spontaneous coagulation of the venous blood in Cachectic and Chronic Diseases, to which M. Olivieri[1] formerly, and M. Bouchut subsequently, directed attention.

Hunter had said in a general manner that the faintness which comes on after hæmorrhages is a salutary phenomenon which allows the blood to undergo coagulation. But he had not penetrated all the points of this important fact.[2] It is to Hewson that belongs the honour of having first indicated and followed in all their details the conditions of the occurrence of this state of the animal body.

Hewson suspects at first—and this sort of intuition was not forbidden to the great observers of the past century[3]—that the coagulating of the blood is augmented in the instances in which *the vital energy is diminished.* In order to verify this fact, he collected, in different vessels successively, all the blood of an animal that was bled to death, and he found that the blood taken immediately at the commencement of the bleeding (that which follows immediately on withdrawing the knife) remained two minutes before coagulating, while the blood collected later, and in proportion as the animal was becoming weaker from loss of blood, coagulated more and more rapidly; until at last, when the animal was in a very weak state, the blood, though entirely fluid on issuing from the vessels, underwent coagulation as it fell into the vessel made to receive it.

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[1] Theses de Paris, Olivieri, 1832; Bouchut, 1845.

[[2] M. Tholozan represents this fact as if it had been noticed by John Hunter. It is very likely that a fact so manifest did not escape that correct observer and sound reasoner; but it was not John Hunter, but William Hunter, his elder brother, who first stated it as one for application in practice. The passage is recorded by Hewson in the following terms:—"And upon this occasion I recollected a remark that I heard, particularly from Dr Hunter, which is, 'That the faintness which comes on after hæmorrhages, instead of alarming the bystanders, and making them support the patient by stimulating medicines, as spirits of hartshorn and cordials, should be looked upon as salutary; as it seems to be the method Nature takes to give the blood time to coagulate.'" Sydenham edition, page 46.

The fact, indeed, was in the time of William Hunter well known, and is mentioned by Cullen. First Lines, DCCCIV.]

[[3] This seems a sort of ironical sneer, not very applicable to William Hewson, and perhaps not very becoming the present author, who seems occasionally to do things by intuition as well as others.]

To these researches I might add, if requisite, the corroborative researches by Thackerah upon the same phenomenon; but science being fixed at present upon this point, I shall not dwell upon it longer. I merely give here the practical conclusion deduced by Hewson himself from these hæmatological researches, without taking the responsibility of all his deductions.

"As hæmorrhages seem to be stopped, partly by a contraction of the bleeding orifices, and partly by the coagulation of the blood; and as the disposition of the blood to coagulate is increased by weakening the body, and likewise the contraction of the bleeding orifices is promoted by the same means; it is therefore evident that the medicines to be used should be such as cool the body and lessen the force of the circulation; and experience teaches us that such are the most efficacious.

"It likewise shews that all agitation of mind, and all bodily motion, should as much as possible be prevented; because they increase the force of the circulation, and are thence unfavourable to the stopping of the hæmorrhage; but that langour and faintness, being favourable to the coagulation of the blood and to the contraction of the bleeding orifice, should not be counteracted by stimulating medicines, but, on the contrary, should be encouraged. And as evacuations weaken the body more when they are sudden, we see a reason why blood-letting should be advisable in hæmorrhages, and why a large orifice should be preferable to a small one, when we want to produce that langour and faintness, or that weak action of the vessels, so useful for the stopping of the hæmorrhage."[1]

Writers have enumerated a certain number of causes capable of destroying the coagulability of the blood. These require to be named, and their influence requires to be estimated anew. Thus, in a man killed by lightning, Davy observed in the heart a soft coagulum, and found that the fingers were still in a state of rigidity, though putrefaction was already well advanced. Sir Charles Scudamore always found the blood coagulated, as a usual fact in animals killed by electricity. Andrew Smith found the blood coagulated in the heart of antelopes which had been hunted to death by dogs. In a hare which had been coursed, Davy saw, according to the report of Mr Gulliver, blood coagulated in the heart. This last observer has published several analogous facts observed in fighting-cocks, in stags and hares run to death.[1] Thus are found verified by experiment the doubts expressed by Andral in 1840, on the subject of the liquidity of the blood, in consequence of mental emotions, concussions, blows upon the pit of the stomach, and even on animals which have been hunted to death. I shall not speak of the action of certain medicinal agents,—mercurial medicines, for instance,—upon the blood. The direct and immoderate effect of these substances is nothing less than solvent. As to alkalies, I shall merely recal the fact, that MM. Andral and Gavarret found that the blood which contains the largest amount of free alkali belonged to a scorbutic patient. M. Fremy has announced analogous facts. It is to be regretted that, up to the present time, the dose of free alkali in the blood is a question unanswered.

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[1] Works of William Hewson, F.R.S., by Gulliver. Hewson on the Blood. *Apud* Sydenham edition; London, 1846. Chapter V., page 77.

CHAPTER SECOND.

ANALYSIS OF NORMAL BLOOD.

The composition of the blood varies in the normal state according to many circumstances, some of which have not yet been the object of precise analysis. It is useful to mention this fact, in order to form, in a general manner, an estimate of the value of the averages which are presented by chemistry.

According to MM. Prevost and Dumas, Lecanu, Andral, and Gavarret, the blood may be considered as consisting, in one thousand parts, of

Water,	790	
Fibrin,	3	
Globules,	127	
Albumen,	68	
Solid matter in the serum,	12	
		1000

Several of these numbers MM. Becquerel and Rodier have slightly modified since 1844. It is important to have their precise data.

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[1] On the State of the Blood and Muscles in Animals killed by Hunting and by Fighting. By George Gulliver, F.R.S., in Edinburgh Medical and Surgical Journal, volume seventieth, No. 177, October 1848, p. 367.

	Mean.	Maximum.	Minimum.
Density of defibrinated blood,	1060·2	1062	1058
Density of serum,	1028·0	1030	1027
Water,	779·0	800	760
Globules,	141·1	152	131
Albumen,	69·4	73	62
Fibrin,	2·2	3·5	1·5
Extractive matters and free salts,	6·8	8·	5·
Fat substances,	1·6	3·255	1·
Seroline,	0·080	0·080	imperfect.
Fat phosphuretted matter,	0·488	1·	0·27
Cholesterine,	0·088	0·175	0·03
Soap,	1·004	2·	0·7

In 1000 grammes of calcined blood there were—

	Mean.	Maximum.	Minimum.
Chloride of sodium,	3·1	4·2	2·3
Soluble salts,	2·5	3·2	2·
Phosphates,	0·334	0·7	1·225
Iron,	0·365	0·633	0·508

These observers (Becquerel and Rodier) have formed this analysis from the examination of eleven persons between the ages of twenty-six and sixty-six years.

Composition of one thousand grammes of Blood in the healthy Female.

	Mean.	Maximum.	Minimum.
Density of defibrinated blood,	1057·5	1060	1054
Density of the serum,	1027·4	1030	1026
Water,	791·1	773	813
Globules,	127·2	137·5	113
Albumen,	70·5	75·5	65
Fibrin,	2·2	2·5	1·8
Extractive matters and free salts,	7·4	8·5	5·2
Fat substances,	1·62	2·86	1·
Seroline,	0·02	0·06	...
Fat phosphuretted matter,	0·464	0·80	0·25
Cholesterine,	0·09	0·20	0·025
Soap,	1·046	1·80	0·725

In 1000 grammes of calcined blood there were—

	Mean.	Maximum.	Minimum.
Chloride of sodium,	3·9	4·	3·5
Soluble salts,	2·9	3·	2·5
Phosphates,	0·354	0·65	0·25
Iron,	0·541	0·575	0·486

These figures are deduced from eight analyses of the blood of females, whose ages varied from twenty-two to fifty-eight years.

It may be remarked, that these analyses form for the fibrin a lower figure than that, which has been generally adopted before they were known, while they raise the numerical proportion of the globules from 127 to 141.

Simon, in two analyses of Venous Blood, found, in one thousand parts, the following proportions.

	Man at 18 years.	Woman at 23 years.
Water, ...	791·900	798·656
Solid residue, . . .	208·1	201·344
Fibrin, ...	2 011	2·208
Fat, ...	1·978	2·713
Albumen, ...	75·590	77·610
Globuline, ...	105·165	100·890
Hæmatine, ...	7·181	5·237
Extractive matters and salts, .	14·174	9·750
Colouring matter in 100 parts of globules, . . .	6·3	5·2

The following table shews to what extent these proportions may vary in the pathological state. It is formed according to the analysis of Franz Simon only.

The water may vary from . . .	880·	to	750.
The fibrin „ from . . .	9·1	to	traces.
The fat „ from . . .	4·3	to	0·7
The albumen „ from . . .	131·0	to	55·1
The globuline, „ from . . .	106·6	to	30·8
The hæmatine, „ from . . .	8·7	to	1·4
The extractive matters and the salts from	16·5	to	7·6

These numbers, however, must not be considered as representing the extreme limits. MM. Andral and Gavarret, for instance, found the fibrin raised to 10·5; Rindstropf to 1·27, *apud* Simon; and Popp, in one case of articular rheumatism, saw it rise to 13·3.

M. Poggiale has endeavoured, by numerous experiments, to determine the composition of the blood of new-born infants. He obtained the following results.

1. The water of the blood of the fœtus presents an average not high, while the proportion of saccharine matter is considerable.

2. The blood of new-born infants is very rich in globules, and poor in fibrin.

3. The quantity of albumen and fat substances seems to be nearly the same in the new-born infant and the adult.
4. The oxide of iron is more abundant in the blood of the new-born infant.

We add with M. Poggiale, that MM. Andral, Gavarret, and Delafond, had previously observed in young animals a remarkable diminution of fibrin, while the globules were very abundant.

Comparison between Arterial and Venous Blood.

Arterial blood contains less solid residue generally than venous blood; it contains also less fat, less albumen, less hæmatine, a smaller proportion of extractive matters, and salts. The globules of arterial blood contain less colouring matter than those of venous blood according to Simon. This chemist found no fixed relation to subsist between the proportion of fibrin and that of blood globules. Sometimes the arterial blood, sometimes the venous blood, contained larger proportions of fibrin and of blood globules. The analyses of Denis, of Lassaigne, and of Hering, corroborate the results obtained by Simon; as to quantity, there is less considerable solid residue of arterial blood; they differ in shewing that the fibrin of arterial blood exceeds in quantity that of the venous blood in the ox and the sheep, while in the horse the fibrin is in excess in the venous blood. (The venous blood of the horse furnishes constantly a buffy coat.[1]) Hering found also that the albumen was in larger quantity in arterial than in venous blood. Autenrieth, Prevost, Dumas, Lecanu, and Schultz, have arrived at conclusions altogether different; for they found that the solid residue of arterial blood exceeds that of venous blood. Letellier perhaps makes the nearest approach to the truth; for the results of his observation are, that no precise rule can be formed on this subject. According to M. Beclard, junior,[2] the venous blood differs from the arterial not only in its physical properties,—that is, colour, temperature,—but further in the proportion of its elements. It contains fewer globules than arterial blood, and a quantity sensibly more considerable of fibrin.

After all, we may say that these differences must be extremely slight; that they amount only to very small numbers; that they must depend so much upon the mode of functional action of the liver, the kidneys, the skin, the lungs, that the smallest irregularity in these functions may cause that an excess of any of the principles may be found, in one case in the venous blood, in the other in the arterial blood. In order to render the difference truly sensible, it would be requisite, for example, to compare the blood of the renal artery to that of the renal vein, and that of the hepatic veins to that of the Portal vein.

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[1] Gulliver's Notes on Hewson. Hewson's Works, Sydenham Edition. P. 40. London, 1846.

[2] Archives de Medecine, 1845.

This has been in part done, and we possess at present analyses numerous enough of blood proceeding from different regions of the body. I adduce, among others, the blood of the Portal Vein, that of the Splenic Vein, that of the Hepatic Veins, and that of the Renal Veins.

Chemical Constitution of the Blood of the Portal Vein.

In some analyses by Simon, the blood of the Portal Vein contains sometimes more solid residue, sometimes less, than the arterial blood; sometimes one of these sorts of blood, sometimes the other contains more albumen. The blood of the Portal Vein differs, however, by a great number of physical characters from the arterial blood and the common venous blood. It is deeper in colour; it coagulates more slowly; the clot is less firm, and has a gelatiniform appearance; after remaining at rest, its surface is covered with a shining coat of fatty globules, according to Simon.

Schultz[1] found that the blood of the Portal Vein is in general blacker in colour than the rest of the venous blood. It does not become red by the action of neutral salts, nor by contact with the air, nor by the action of oxygen. It undergoes little or no coagulation. Its solid parts contain almost double the amount of fat of that found in the blood of the arteries and other veins; the proportions are shewn by the following numbers.

	Fat matters.
Blood of the Portal Vein, .. .	1·66
Arterial blood, . . .	0·92
Venous blood from veins at large, .	0·83

The fat of the blood of the Portal Vein is of a blackish-brown colour, unctuous; that of arterial blood and the general venous blood is white, or yellowish-white, crystalline.

According to M. Beclard, the blood of the Portal Vein (the mesenteric trunk), presents, in the proportion of its elements, very extensive variations in reference to the process of Digestion.

During the first periods of digestive absorption, the quantity of albumen is considerably increased; the quantity of globules considerably diminished.

In the periods following this absorption, the quantity of globules is considerably increased, the quantity of albumen considerably diminished.

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[1] Rust's Magazin, Band xliv. 1835.

The blood of the Portal Vein (superior mesenteric trunk), does not contain a more considerable proportion of fat matters than the blood of the general venous system.

The Blood of the Splenic, Hepatic, and Renal Veins.

According to Kölliker, the blood of the Splenic Vein differs from ordinary venous blood by the deeper colour of the serum, the great number of colourless globules, and the presence of globules of a peculiar nature. (*Mikroscopische Anatomie*, Band ii.)

Otto Funke found, that, under the microscope, the blood of the splenic vein of the horse differed in a remarkable manner from the blood of the general sanguiferous system, whether taken from the jugular vein or from the arteries. The red corpuscles did not form rolls or piles, but scattered and irregular, or small rounded and angular agglomerations; the globules were smaller than those which were derived from other kinds of blood. The white globules, which were extremely numerous, amounted, in one instance, to the fourth or the third part of the red globules. (*Henle's Zeitschrift*, 1851, p. 172.)

The blood of the hepatic veins is still deeper in colour than that of the portal vein; its fibrin may be separated from it without difficulty by beating, and, after this separation, the blood still possesses the properties of forming itself into a soft mass, and of furnishing a gelatiniform clot, the serum of which is separated. From the analyses made of the suprahepatic blood, Simon concludes that the blood is more rich in solid materials than that of the arteries, of the veins, and of the Portal Vein, and that it only contains less fat, less fibrin, less globuline, and less colouring matter than the last of these sorts of blood.

It is important to notice, in reference to the last stated fact, that the blood of the Hepatic Vein contains only 4·8 per cent. of colouring matter, while that of the Portal Vein contains 5·4 per cent. The difference is a little more than half a unit.

The blood of the renal vein was a great deal deeper in colour than that of the aorta; but it was impossible to obtain fibrin from it by beating. Nevertheless, this blood, when left to rest, was converted into a gelatinous mass. Upon examining this blood by the microscope, the globules were found broken down into amorphous masses. Analysis gave less water, more solid residue, and more albumen. The quantity of fibrin was too small to be ascertained.

After the modifications of blood, according to sex, age, different regions of the vascular system, comes the examination of the modifications of this fluid connected with the quantity and the quality of alimentary articles. On this subject we find some valuable facts in the Memoir of Nasse upon the Influence of Nutriment upon the Blood, in the writings of Mulder, and in the Article BLOOD of the Chemistry of M. Lehmann. The following table, which I borrow from Dr Bence Jones, presents a summary of almost the whole inquiry.

Effect of Alimentary Articles upon the Albumen, the Fibrin, the Salts, and the Fat of the Blood.

	In 1000 parts of Blood.
Albumen,—Horse fasting,	66·8
„ after feeding,	90·8
„ Man on mixed food,	53·2
„ „ animal food, 5th day,	58·7
„ „ „ 14th day,	62·7
„ „ vegetable food, 14th day,	51·0
Fibrin,—Man confined to diet,	1·8
„ „ on animal food, 3d hour,	1·5
„ „ „ 5th hour,	1·6
„ „ „ 6th hour,	1·5
„ „ „ 7th hour,	1·4
„ „ „ 8th hour,	1·8
„ A dog after eleven days of abstinence,	1·5
„ „ nine days of abstinence,	2·7
„ „ on abundant animal food,	2·2
Water,—After animal food,	78·4
„ vegetable food,	79·2
Salts,—Twenty-four hours of strict diet,	6·9
„ Eight hours after animal food,	8·2
„ „ vegetable food,	7·7
Fat,—Twenty-four hours of regulated diet,	2·0
„ „ „ on animal food,	2·9
„ „ „ vegetable food,	2·1
„ Eight days of regulated diet,	1·7
„ Eleven days of regulated diet,	1·4

Along with these different facts there is another list perhaps more prolific in pathological deductions. I mean the relation which subsists in the different animal species between the quantity of globules on the one hand, and on the other, the temperature and the rate of the movement of the heart.

Animals.	Globules.	Medium Temperature.	Pulse.	Respiration.
Pigeon,	15·57	107·5	136	34
Common Fowl,	15·71	106·7	150	30
Duck,	15·01	108·5	110	21
Crow,	14·66	108·5	110	21
Heron,	13·26	111·2	200	22
Ape,	14·61	95·9	90	30
Man,	12·92	98·6	72	18
Guinea Pig,	12·81	100·4	140	36
Dog,	12·38	99·4	90	28
Cat,	12·04	101·3	100	24
Deer,	10·20	102·5	84	24
Hare,	9·38	100·4	120	36
Horse,	9·20	98·2	56	11
Sheep,	9.20	100·4	...	...
Ox,	10·50	99·5	38	...
Carp,	2·10	51·1	20	...
Tench,	1·40	51·4	...	...
Green Toad,	2·20	51·4	77	...

I pass over in silence many details relative to physiological Hæmatology; understanding all the elementary details upon the composition of the blood to be well known.

It would be requisite to make a long chapter upon the different processes of analysis employed at the present time in researches on the pathological states of the blood. It would be requisite to explain the relative value of these different methods, and to prove the limits of error within which the results given by the best observers may oscillate. This is a task rather chemical than medical, and I feel myself unable to undertake it in this place.

I only refer to the singular results obtained in different modes of analyzing the blood by MM. Marchal de Calvi,[1] Corne,[2] and Abeille,[3] to which these observers have wished to give a certain pathological signification. These experiments are interesting to be consulted as partial facts tending to shew, that different analytical processes may produce in the numerical results considerable changes, although the true composition of the blood is in no manner modified. Under this point of view it is useful to remind observers, who may be induced to repeat these experiments, of the results already obtained by Davy, Gulliver, Polli, upon the influence of temperature upon the coagulation of the blood, upon the effect of agitation upon the coagulation of the fibrin, and the experience of chemists upon the difficulty of collecting all the fibrin when it is imprisoned in the clot, or when violent motions, as in the experiment of Davy have reduced it into fine powder.

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[1] Gazette Medicale, 1849, p. 666, Augmentation of Fibrin of the Blood by heat.

[2] Ibid., 1850, p. 229, Diminution of Fibrin in the Blood under the influence of motion.

[3] Ibid., 1851, p. 207, Causes of Fibrination and Defibrination of the Blood in different Pathological states.

In order to shew to what extent this interpretation is true, I shall state as an experimental proof, the results obtained by Johann Hittorf[1] in two analyses of the blood, in which the fibrin has been extracted from the two halves of one quantity obtained by one and the same bloodletting, collected in alternate fourths, and the interpretation which this judicious observer puts upon these results.

" Always in my analyses, therefore, I employ water containing the above mentioned proportion of salt. When by this method the purest fibrin is washed, the weight of this and of what is separated from blood and cruor ought to be the same. But in point of fact, after the experiment was twice performed with blood, drawn at the same time, and from the same individual, I found the following results. The fibrin was treated in both methods for the same time and with the same solution.

In the first instance:

In one thousand parts of blood the weight was—	Grammes.
Of fibrin separated by moving (agitating) the blood,	1·455
Of fibrin separated from the *cruor*, . .	1·381
Difference between the two, . .	·074

In the second instance:

In one thousand parts of blood there were found—	Grammes.
Of fibrin separated by agitating the blood, .	1·929
Of fibrin separated from the *cruor*, . .	1·865
Difference between the two, . .	0·64

" The difference here remarked certainly proceeds from the fact which we have above mentioned. Some particles, if the very thin fibrin of the *cruor* is washed, penetrated through the substance of the cloth, or, what is more probable, were so closely interwoven with the very threads of the cloth, that they could not be found by the most diligent search. It is always most difficult to collect all the elements of whitish fibrin dispersed through the cloth; and after every part of the cloth has been most accurately examined, yet if you go over it a second and a third time, you will find individual small filaments interwoven with the intervals of the cloth. These inconveniences and sources of mistake are not to be dreaded by our method. The fibrin separated by agitating the blood contracts itself into a ball, and the larger particles only adhere to the cloth."[1]

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[1] Theses, Bonnæ, 1846.

On the Blood during Pregnancy.

By the side of those facts, which belong to the physiological condition of the blood, I shall place another, which cannot justly be attached to the pathological state. I refer to the condition of the blood at different periods of pregnancy.

MM. Andral and Gavarret had perfectly made known the conditions of the blood, which are at present designated by the name of *hydroœmia* of pregnant females. Among thirty-four blood-lettings performed during the different months of pregnancy, in one instance only the globules rose above the physiological medium (145); in one other instance they presented the physiological mean (127). In the other thirty-two instances, they remained below the mean, varying in six cases from 125 to 120, and in the other twenty-six cases from 120 to 95.

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[1] Itaque semper in analysibus aquâ utor, supradictam salis copiam continente. Quâ ratione quum purissima eluatur fibrina, *pondus ejus, et e sanguine et e cruore separatae,* idem esse deberet. Sed re ipsa, experimento in sanguine, eodem tempore eodemque in homine misso, bis instituto, haec inveni. Fibrina utraque ratione per idem tempus eademque dissolutione tractata.

In casu primo.		
Ex mille sanguinis partibus ponderabat:		
Fibrina, *sanguine movendo separata,* . . .	1·455	grammos.
... Ex cruore secreta, ...	1·381	...
Discrepantibus inter se, . . .	0·074	
In casu altero.		
In mille sanguinis partibus inveniebantur:		
Fibrinae, sanguini movendo separatae, . .	1·929	grammi.
... Ex cruore secretae, ...	1·865	...
Discrepantibus, ...	0·064	

"Quæ quidem differentia ab eo certè proficiscitur, quod supra jam commemoravimus. Particulæ nonnullæ, si fibrina tenuissima cruoris eluitur, per ipsum linteum penetrarunt, vel, quod probabilius erit, ipsis lintei filis tam accurate contextæ sunt, ut summa diligentia nequeant inveniri. Difficillimum semper est, omnia fila fibrinæ albidæ, per solum linteum dissipata, colligere; et quacumque panni parte accuratissime pervestigata, tamen si iterum tertiùmque quæsiveris, singula, ipsis intervallis intexta fila parva invenies. Quæ omnia nostra ratione non sunt timenda. Fibrina sanguine movendo separata in globulum se contrahit et majores solum particulæ linteo adhærent."

As to the fibrin, it varied in these analyses, according to the period of gestation, between 2·5 and 4·3.

The analyses of MM. Becquerel and Rodier subsequently, and especially those of M. Regnauld, have elucidated in all its parts this interesting phenomenon of the *Anæmia* of pregnant females. Hence results, as M. Cazeaux among the first perceived, one of the most valuable practical data of Hæmatology, which may be expressed in the following terms. The dynamic disorders of pregnancy which may be ascribed to plethora, are those of *Chlorosis*. These data derive their confirmation from the exact analysis of symptoms, their clinical interpretation, and the results of treatment.

CHAPTER THIRD.

ON THE ALTERATIONS OF THE BLOOD CONSIDERED IN REFERENCE TO HUMERAL PATHOLOGY.

PHLEGMASIÆ; PYREXIÆ; PLETHORA; ANÆMIA.

ARTICLE FIRST.

GENERAL OBSERVATIONS.

Before commencing the actual study of the Alterations of the Blood, it was requisite to fix the boundaries of the department in which we were to arrange all those alterations, and the method which was to be adopted for their exposition. Ought we to follow the analytical order, which is also the Chemical, and, taking one by one all the principles of the blood which have been subjected to analysis, to shew their variations in different diseases,—that is to say, to trace the pathological history of all these principles? This is what the writers of the German School have done; it is what has been repeated by some English pathologists. These authors have thus arranged diseases in different categories, which I regard as quite artificial.

Simon of Berlin, for example, who has a great reputation in Germany, has divided the chemical study of the blood in Diseases into four chapters, in the following manner.

1. *Hyperinosis* (Υπεϱ, Ις, ινος, *fibra*), the condition in which there is an excess of fibrin, and a corresponding diminution of red globules.

2. *Hypinosis;* the state in which the quantity of fibrin is smaller than in the normal blood; or that state in which, if the fibrin is in normal quantity, its proportion is nevertheless

smaller in relation to the globules than in the state of health.

3. *Spanæmia* (σπανος, *rarus; pauper;* the state in which the blood contains an excess of water, none of the constituent particles exceeding the normal mean.

4. *Hetero-chymeusis;* which comprehends those states of the blood in which there is present a substance which is not found in the normal state of the blood; when, for instance, the blood contains *urea* in appreciable amount, *sugar*, biliphæine, fat, purulent matter.

Dr Charles Williams, in his *Principles of Medicine*, divides the diseases of the constituent parts of the blood, into Diseases from Excess of the red globules, the fibrin, &c.; Diseases from Diminution of these principles; and Diseases from alteration in these principles. This is a step in advance of the classification of Simon; but it is still defective in a pathological point of view. There exists no disease from excess, diminution, or alteration of the red globules; any more than there are diseases from *Euplastia*, *Kakoplastia*, or *Aplastia* of the Fibrin; any more than there are diseases of carbon or of oxygen. In all these classifications the authors lose sight of the fact that Pathology is instituted for the study of diseases, and that every classification which takes not for its foundation nosological distinctions and the study of organopathic elements, commits the great error, as M. Piorry shews, of departing from diseases, and preventing any practical approximation.

Mr John Simon, in his lectures delivered in London in 1850, divides this subject in the following manner.

1. Diseases in which poisonous elements proceeding from without are introduced into the blood;—as poisons, miasmata, virus of various kinds.

2. Diseases depending on an augmentation, a diminution, or an alteration of excreted products;—as carbonic acid, bile, urea.

3. Diseases in which a modification of the special and constituent parts of the blood has taken place,—primary or idiopathic diseases of the blood. These are the most important of all,—Plethora, Chlorosis, Phlegmasiæ, &c.

Here is still the same fault and the same error as in the preceding classifications.

I know no classification of the pathological changes incident to the blood more complete than that which has been proposed and developed by M. Piorry in his Treatise on the

ANOMÆMIÆ; a work which, in my opinion, is the most complete that we at present possess upon the subject.[1]

M. Piorry considers successively, under the generic title of ANOMÆMIÆ, that is, Pathological Alterations of the Blood; *Panhyperæmia*, or *Plethora; Panhypæmia*, or general *Anæmia; Hydræmia, Hypochalybæmia, Hypohydræmia*, diminution in the proportions of the serum; *Aplasticæmia*, scurvy; *Anhæmatosis; Hæmitis;*

TOXÆMIA, which includes the following sub-genera:—*Cholæmia, Uræmia, Galactæmia, Pyohæmia, Phymohæmia*, tubercular diseases, *Carkinohæmia, Cancer; Septicæmia*, putrifactive poisons; *Phytonæmia*, vegetable poisons; ZOOTOXÆMIA, animal poisons; the contagious *Toxæmiæ.*

These last include plague, glanders, smallpox, measles, scarlet fever, syphilis, and similar diseases.

Lastly, TOXÆMIÆ caused by chemical agents.

I could neither profess nor desire to be more complete in a Thesis upon the Alterations of the Blood; but here I have to shew the present state of knowledge in Hæmatology, without giving a complete treatise on the subject.

Not adopting the chemical order of Franz Simon and Dr Charles Williams, because I believe it to be bad; nor the physiological order of Mr John Simon, which I look upon as incomplete; nor the order which accords best with the doctrine of organopathic elements, because I think it inapplicable to my subject; I must select that which I shall denominate the Nosological Order. As to other points, the reasons which operate most in favour of this method are, that this exposition allows me to give at the outset some general opinions upon the actual state of our knowledge in Hæmatology. It is upon the imperfections of this knowledge that I proceed for the adoption of my method of exposition.

The most general and the most correct result to which researches in Hæmatology have led is the exact, precise, and distinctly-defined separation of the PHLEGMASIÆ from the Idiopathic Fevers. Notwithstanding this, however, as to those two tribes of diseases, the recent advances in knowledge do not permit us yet to say, whether both or one of them belong to the head of alterations in the solids or in the blood. The same is the case, in my opinion, as to Plethora, as to Chlorosis, as to Scurvy; the same as to Cholera, as to Yellow Fever. I intentionally associate here all those morbid states in which there is evidently present a great alteration in the fluids; a change which I recognize, and which I shall forthwith endeavour to analyse with all the accuracy and precision of which the subject admits. But from the fact that a change in the fluids exists, are we to conclude that the fluids thus altered constitute the disease; and, for instance, instead of saying Plethora, can we say excess of globules? instead of saying Chlorosis, can we say diminution of globules or of iron? in place of saying Scurvy or Purpura, can we say *Hypoplasma*, *Cacoplastia*, *Aplastia*, as we say Pneumonia Pericarditis, Hepatitis, Apoplexy?

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[1] Traité de Medecine Pratique et de Pathologie Iatrique ou Medicale. Paris, 1847. T. iii., p. 21.

This question, which touches the generalities of Hæmatology, and the foundations even of the Humoral Pathology, possesses a sufficient degree of importance to require us to pause here for one moment, in order to attempt its resolution with the lights furnished by Hæmatology itself.

Broussais and Fletcher considered the blood not as the *seat*, but as the *vehicle* of different diseases. In our days, with the progress of Pathological Anatomy and the Humoral Pathology, with the lights which Physiology throws upon it, it is almost impossible to affirm, that no disease has its exclusive seat in the blood. In the most simple affections, and those which are confined to the smallest number of tissues, it is often difficult to be certain that the disease does not arise from other alterations more concealed, and which escape our notice, or of which we obtain only an incomplete glimpse. With greater reason this is applicable to the diseases of the blood, which may indeed be considered in itself as an organ endued with a position liable to be changed by the reciprocal action of its different elements, but which being the terminal end, and the common band of all the other organs, undergoes the influence of the slightest modifications which take place in all points of the economy, and which are ordinarily affected by changes in the composition of this fluid.[1]

Hitherto, and so long as we shall consider, in the blood only the principles which proceed incessantly from the decomposition or the nutrition of the organs, it will be impossible to establish a category of diseases peculiar to this fluid, without the Solidists justly protesting in reference to their doctrines. Let us remark, further, that if this doctrine of the proper vitality of the blood is once admitted, of the blood as organ and function, we openly admit the validity of the doctrine of Solidism, which is nothing but Organic Pathology, In my opinion it is the only manner in which we can contemplate in time-coming the Diseases of the Blood. To have determined with precision the changes which this fluid undergoes in different diseases, as has been done by MM. Andral and Gavarret, and all those who have followed their steps, is not at the present time sufficient. We observe at this time, that these data do not resolve a great number of questions on pathological physiology, without which science cannot advance, the lights of which alone would deliver medical practice from the empiricism in which it is at present immersed.

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[1] The author here becomes confused, and expresses himself in a manner which is not very intelligible. The passage is therefore omitted.

When we know of Rheumatism only the articular lesions, and of Typhous Fever only the intestinal alteration, we cannot be deceived upon the tendency and signification of these maladies. At the present time when the discoveries which gave lustre to French Medical Science, have associated with these two diseases, on the one hand the alterations in the serous membranes of the heart (*Pericardium* and *Endocardium*) and the hyperfibrination of the blood, on the other side the lesions almost general of the solids and a poisonous change in the blood, then we know what accuracy has been acquired in Diagnosis, Prognosis, I may add even Therapeutics.[1] What has been done in these two instances as to the lesions of the solids, it is necessary to undertake for those of the blood. The great point is to determine whether these alterations are the cause or the effect of the lesions on the solid tissues. Undoubtedly it is important to know that in all Inflammations the fibrin of the blood is increased in quantity, that in Chlorosis there is a diminution of the globules, and in Plethora an excessive amount. But these three indications which I choose among the most significant, we ought to accept only as symptoms of a morbid condition; for at present it would be impossible to answer the following questions.

1. In what manner the blood is over-fibrinated in Inflammations; 2. Whether Chlorosis proceeds from disorder in the digestive organs, and in the Lymphatic apparatus charged with the assimilation of alimentary articles, and the formation of globules; 3. Is Plethora owing to an alteration in these functions entirely opposite, or a defect in the depuratory organs of the blood?

The blood undergoes in the substance of the animal tissues continual transformations, which may be regarded as an expression of its vitality. The production of globules and the formation of blood are intimately connected with Nutrition. When Nutrition is bad and insufficient, the quantity of blood or rather that of blood globules is diminished; when nourishment is good and abundant, the contrary effect is produced. In the first instance, the vital energy is depressed; the secretions and the excretions are diminished; animal heat falls; while in the second case the opposite phenomena are observed. In the normal state there is an equilibrium between the production and the consumption of the globules of blood. The reparatory substances are prepared, and to a certain degree assimilated before being received into the blood. The vital energy of the globules continues to be exerted even during absolute diet, and carbonic acid and urea are formed, though their quantity diminishes gradually in the direct ratio of the diminution of the red globules. Moreover, the quantity of carbonic acid and that of urea is diminished by retardation, and increased by acceleration of the circulation; and the animal temperature is so much more elevated, as the proportion of globules is more considerable, and as the circulation is more active. This is what takes place in BIRDS, while the contrary is observed in the AMPHIBIA. In Chlorotic females and in aged persons, the temperature is diminished as well as the excretion of urea; while in inflammatory diseases and after violent exercise, the temperature is raised, and there is a relative or absolute increase in the amount of urea, even notwithstanding the absence of all nitrogenous food, in instances of inflammation. If we knew how much and in what manner the pulmonary exhalation is modified in different diseases, especially in relation to the quantity of carbonic acid; if we knew whether the carbonic acid is always increased in proportion to the urea, and in certain cases to that of the uric acid; if further experiments had been performed regarding the effect of different diseases and different kinds of regimen upon the bile; we should then have a more solid foundation for chemical inductions, and these determinations would supply to medicine positive information of great importance.

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[1] [It is quite unnecessary to say any thing as to the strange delusion under which the author labours as to the pathological changes taking place in Rheumatism and Typhous Fever, it should have been Typhoid Fever, being the exclusive discovery of French Pathologists. The error is so general that it would only give offence to point it out. All that we say is, that it originates in ignorance of the writings of English Physicians.]

ARTICLE SECOND.

ON THE BLOOD IN THE PHLEGMASIÆ.

In considering those large classes of diseases in which the blood presents the most remarkable alterations, I purposely invert the order followed by the author of Pathological Hæmatology. I shall examine the blood in the PHLEGMASIÆ previous to considering the fluid in the Idiopathic Fevers.[1]

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[1] [A considerable mistake in Nosology is habitually made by the present and many other writers, in speaking of the PHLEGMASIÆ as distinct from and opposed to the PYREXIÆ. The PYREXIÆ constitute the class; and the *Phlegmasiæ* are the order, the second order in that class. Instead of the term PYREXIÆ or PYREXIES, the word employed on this occasion ought to be FEBRES or FEVERS, the first order in the class PYREXIÆ. This is a mistake not unusual with many who pay no attention to the accurate nosological distinctions originally made by Cullen.]

In man the fibrin in an acute inflammation, well established, oscillates ordinarily between the numbers of six and eight; sometimes it rises to eight and nine; seldom it attains the amount of 10 (Andral).

The instances in which the fibrin does not rise above five, relate to *plegmasiœ* acute but slight. The fibrin oscillates in these instanees between four and five; it never sinks below four.

In two instances of *Phlegmon*, the numerical amount of fibrin was 4·7 and 4·5.

Among eighty-four (84) bloodlettings performed in the course of pneumonic attacks well marked, in seven instances the fibrin oscillated between 4 and 5; in eleven instances between 6 and 7; in fifteen cases between 7 and 8; in seventeen cases between 8 and 9; in nine instances between 9 and 10; in six instances it rose to 10 in 1000 parts. The mean, it will be remembered, is according to Becquerel and Rodier 2·2.

In the inflammations of the serous membranes, M. Andral obtained identical results.

In a first series of observations in which the disease was of recent date or of long standing, and attended with fever, was proceeded always to be aggravated, they find the blood loaded with an excess of fibrin, giving as the minimum 4, and as the maximum 8·4. In another series of facts, in which the inflammation seems to be arrested, in which the pulse has become less frequent, and in which no more of the disease remains except an effusion more or less considerable, the blood then ceases to present an excess of fibrin.

In articular rheumatism, a morbid condition is well marked, and upon the nature of which so much discussion has taken place in our days, the same law of inflammatory diseases holds good. If the disease is acute, the fibrin augments in a constant manner, says M. Andral, and we find in forty-three bloodlettings, once the number 4; in six instances the number 5; in fifteen instances, the number 6; in thirteen instances, the number 7; in three instances, the number 8; in three instances, the number 9; and in two, the number 10. If the rheumatic attack be less acute, the fever less intense, the amount of fibrin rises not so high; in six instances of this kind, it oscillates between the numbers 4 and 5. *Lastly*, if the rheumatism be entirely chronic, the quantity of fibrin does not exceed the physiological number(2.)

Analogous results are given in acute Nephritis; in inflammation of the Ganglions; in Softening of the brain.

In the inflammations of the Mucous Membranes, the same fact presents itself; and here also the quantities of fibrin appear to be in proportion to the different degrees of inflammation.

In instances of *Bronchitis*, acute and extended, the fibrin rises to the numbers 6, 7, and 9, according to Andral; while in chronic cases there is no augmentation of the fibrin. In four instances of mercurial *Stomatitis* the fibrin was found to be 4·5, 5, 8·4; and 6·6. Lastly, in inflammations of the different portions of the mucous membrane of the alimentary canal, it was found to be represented by the numbers 5, 6, and 7.

In cases of Cystitis, in inflammation of the utero-vaginal mucous membrane, the fibrin varies between 4, 5, and 7.

Acute inflammations of the skin, for instance Rose, act as those of the mucous membranes, as those of the serous membranes, as those of the cellular tissue, and give rise in the blood to that alteration so small in appearance, but so constant and so characteristic, which is interpreted by increase in the amount of Fibrin.

Subsequent to the date of the labours of MM. Andral and Gavarret, MM. Becquerel and Rodier have established the same fact of the increase of Fibrin in Inflammatory Diseases. We give the results of their analysis in full.

Mean Composition of the Blood in the Phlegmasiae.

	In Men.	In Women.
Density of defibrinated blood,	1056·3	1054·5
——— of the serum,	1027·	1026·8
Water,	791·5	801·
Globules,	128·	118·6
Albumen,	66·	65·8
Fibrin,	5·8	5·7
Extractive matters and soluble salts,	7·	7·2
Fat substances	1·742	1·669
Seroline,	0·020	0·024
Phosphuretted matter,	0·602	0·601
Cholesterine,	0·136	0·130
Soap,	0·984	0·914
In 1000 grammes of Calcined Blood there are—		
Chloride of sodium,	3·1	3·
Soluble salts,	2·4	2·7
Phosphates,	8·448	8·344[1]
Iron,	0·490	0·480

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[1] In the original, this is 0·344. But the number is manifestly a typographical error for 8·344.

Upon comparing these tables with those which have been given from the same chemists, and which represent the mean composition of the blood in the state of health, it may be remarked, that the only elements of the blood which present variations in their quantity, are the fibrin, the cholesterine, and the albumen.

The augmented principles are the Fibrin and the Cholesterine. " This increase in the numerical amount of fibrin in Inflammatory Diseases takes place, whatever otherwise be the composition of the blood, and the proportion of the other principles contained in this fluid. Thus, the Fibrin may be represented by a very elevated number, when the globules are on the contrary very much diminished in quantity. The increase of Fibrin is greatly more remarkable in the Acute *Phlegmasiae*, than in the Chronic, in which it is slight and may even not take place." Of the increase of the Cholesterine, it is difficult to find the cause.

[The increase of cholesterine is connected with the derangement and suspension in the process of secretion. In the *Phlegmasiae*, the secretions are all more or less diminished; and hence the materials of the secreted products exist in the blood in increased proportion. Not only is cholesterine increased, but bilin, taurine, urea, and other articles are found in the blood, which either ought not to be in it, or to be in it in very minute and almost imperceptible quantity.]

When a fact appears with this uniformity in the phenomena of this kind of inflammation;—when we observe it following in all its variations the leading phenomenon, undergoing modification in proportion to it, and obeying it in all its variations;—we are naturally led to think, that the fact is dependent upon the process, that it arises from the same laws, that, in one word, it is a natural effect. This inference M. Andral admits, yet without attempting to interpret the relation of the cause to the effect. He has satisfied himself that the increase in the Fibrin does not pre-exist in the *Phlegmasiae*; he has seen that an excess of Fibrin is developed consecutively to phlegmasial attacks induced artificially; and he hence deduces the inference, that increase of Fibrin is one of the effects and one of the best signs of the inflammatory process. As to the manner in which this increase of fibrin is brought about, " there are in this inquiry unknown things to be discovered; it is requisite to discover that mysterious relation, the necessity of which is proved by its regularity, which connects, in diseases, alteration in the solid tissues with alteration in the blood."

This is the language of Pathology even at the present time. Since the time of Andral we have not made one single step in advance in this question. The chemists, it is true, have endeavoured to explain it in their fashion in different modes. We shall quote here, as an attempt at a chemical explanation, the theory of Mulder, which has given rise to many modifications, and which has made much noise in Germany, and even in France. The reader will see in it perhaps some ideas worthy of being adopted. But the foundation of it I regard as pure hypothesis.

Mulder regards Inflammation as a more elevated degree of oxidation. The albumen of the blood, he informs us, which furnishes only the tritoxide by boiling, takes no share in this change; we may conclude that it is effected by the fibrin alone, which is susceptible of absorbing the oxygen of the air and easily converted into binoxide and tritoxide of Proteine. During the presence of inflammation, there is a great excess of these oxides in the blood. Respiration may be regarded as a real oxidation of the blood, or rather of the Proteine; and in Inflammation, in which the blood contains a larger quantity of Binoxide and of Tritoxide of Proteine than in the state of health, this body becomes more strongly oxidated.

In all instances of accelerated respiration, in Fevers for instance, inflammation is easily developed after fatigue or violent effort. Each febrile paroxysm ought necessarily to determine the formation of a larger quantity of oxidated proteine in the economy; and each augmentation in the quantity of proteine ought to produce inflammation, which in its turn will give rise to Fever. Hence it results, that stimulating aliments and drinks which accelerate respiration, or even cold air which introduces a greater quantity of oxygen into the lungs, often give the first impulse to the occurrence of inflammation in the organism. The buffy crust is formed when the oxides of proteine predominate in the blood; when they are accumulated in a particular part of the organism, a local inflammation is produced. In this last case there are formed morbid products, such as false membranes which are composed of oxidated proteine. Inflammation must be opposed by forcibly diminishing the quantity of Tritoxide of Proteine, and preventing its formation in the lungs. Blood-letting acts as an antiphlogistic remedy, by diminishing the quantity of Tritoxide of Proteine. An increase in the secretion of the intestinal mucous membrane produces indirectly the same effect, by accelerating the change of substance in the body, and consequently by favouring the destruction of a far greater quantity of proteine and its oxides.

In recapitulation, all that our hematological researches have taught us upon the Inflammatory Diseases may be stated in the following terms. One fact which is connected with a discovery, is the increase of the fibrin in the blood; but does this fact, viewed by itself, throw any new light upon the history of the inflammatory diseases, or explain better than formerly the effects of remedial agents?

In another chapter we shall consider at length the effects of bloodletting upon the organism. Here, in reference to the *Phlegmasiae*, we shall say merely in a general manner, that the practical application which flows from the knowledge of Hematology, the only one perhaps, is that it is not necessary to hesitate to adopt at the outset, in the treatment of these disorders, the only great disturbing power which the physician possesses, namely bloodletting. When this indication is once established, Hematology teaches us to put no confidence in that false crust, the thickness of which increases in every successive bloodletting; and Hematology furnishes us with the means of persevering, to a certain extent, in the use of bloodletting, by shewing us that in the *Phlegmasiae*, bloodletting acts less than in other affections, in diminishing the proportions of the solid parts of the blood.

In this condition of the blood, mercurial medicines, antimonials, and the alkaline salts may be useful as purgatives, and other means of augmenting the most important secretions by the quantity of solid matters which they become the means of eliminating. According to the experiments and the inferences of Dumas and Liebig, saccharine, amylaceous, and gelatinous aliments, which diminish the quantity of fibrin and albumen in the blood, are those also which are most efficacious in abating and removing inflammatory diseases.

ARTICLE THIRD.

THE BLOOD IN FEVERS.[1]

It is only after having formed a perfect understanding of the part which the excess of Fibrin performs in the *Phlegmasiae*, that we can attempt to study the state of the blood in Fevers. It is only after having studied the diseases which are distinguished by the presence of a positive sign, that we can analyze those which present almost none but negative characters in a hematological point of view. I give at first the analytical details.

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[1] The author commits here the same mistake as that already noticed in calling FEVERS PYREXIAE. It will be seen that he includes here the EXANTHEMATA.

MM. Andral and Gavarret examined analytically the blood; 1st, in the preliminary stage of Continued Fevers (eight analyses); 2d, in Continued Fevers and Typhoid (twenty-one analyses); 3d, in Continued Fevers, complicated in their course with some inflammation (eleven analyses); 4th, in Typhoid Fever (twenty cases, forty-nine analyses); 5th, in Smallpox (seven cases, fourteen analyses); 6th, in Measles (seven cases, nine analyses); 7th, in Scarlet Fever (two analyses). From these researches the author draws the conclusion, "that in not one of these seven tribes of diseases, does the fibrin present an increase in quantity, except in some instances which were complicated with an inflammatory attack; that often, on the contrary, the fibrin diminishes in quantity either absolutely or relatively; the globules may preserve their normal state; in a certain number of instances they increase according to a very remarkable rate; and they never diminish but in consequence of circumstances the influence of which is superadded to that of the disease.

In the Essay on Haematology, M. Andral adds the following statement. "But is it indiscriminately and as it were at random, that the Fibrin will shew itself in the Febrile Disorders, either in normal quantity, or in a proportion infinitely smaller than in the physiological state? Certainly not; and in this respect very exact general principles may be laid down." "The diminution of fibrin exists not necessarily in any Fever; but it appears incontestible, that the specific causes which give rise to diseases of this order, act upon the blood in such a manner, that they tend to destroy in it the spontaneously coagulable matter. If the cause acts with a small degree or energy, or if the economy has good resisting power, the destruction of the fibrin is not accomplished; if, on the other hand, the cause continues to act with all its intensity, and the forces of the organism are deficient, the destruction of the fibrin will be commenced either from the first onset of the disease, which is very rarely the case, or at a certain time after it has taken its origin."

If we carefully consider the sense of these words, and if we examine in detail the results of the analyses published by MM. Andral and Gavarret, we shall see that several writers have ascribed to these authors more than they had said on the subject of the distinctive signs which hematological knowledge furnishes for the destruction of the Idiopathic Fevers. These signs are at present negative.

This will enable us to understand how MM. Becquerel and Rodier, upon analyzing the blood in Typhoid Fever, have arrived at the following inferences.

1. The globules may remain in normal quantity, if the bloodletting is performed at a period not far removed from the commencement of the attack, and when the disease is still slight, although at a later period it may be aggravated.

2. The albumen of the serum diminishes in a remarkable manner. This diminution is more considerable in the very severe cases, especially if previous bloodlettings have been performed.

3. The fibrin generally remains at its normal number at the commencement of Typhoid Fever, at the period of the first bloodlettings; it diminishes in a remarkable manner when we come to perform further bloodlettings. We are hitherto ignorant of the law which regulates this diminution.

"In conclusion," add the same observers, "the blood in Typhoid Fever presents absolutely no striking, positive, constant character; and, excepting perhaps some exceptional instances in which the fibrin is diminished, all the modifications which we have just established in the blood may be created and explained by influences different from those which produce and arise from this serious disorder."

This conclusion, which was drawn by MM. Becquerel and Rodier in 1844, is still at the present time the most advanced written expression of science. There has come to my knowledge no work which invalidates these inferences, or transfers to another field in another direction the analysis of the blood in Idiopathic Fevers, and in Typhoid Fever in particular.

The only general inferences which can be justly established on this head are the following.

1. In an etiological point of view, I shall say that the distinction fixed between the PHLEGMASIAE and the IDIOPATHIC FEVERS by M. Andral subsists completely, because it is founded not so much upon positive characters derived from the constitution of the blood in the Idiopathic Fevers, as from negative characters of this sort of blood compared with the overfibrinated blood of the Phlegmasiae.

2. In a diagnostic point of view, chemical analysis might furnish, were it more expeditious, important data, by fixing them accurately upon the absence or the presence of the phlegmasial character of the blood.

3. Lastly, in a therapeutic point of view, I have not in this place to notice the treatment of Idiopathic Fevers. I only say, that the results of chemical analysis, and the study of the effect of successive bloodlettings upon the composition of the residual blood in Typhoid Fever, for instance, appear to me not to contraindicate moderate depletion of the vascular system, especially at the commencement of this disorder.

ARTICLE FOURTH.

OF THE BLOOD IN ANAEMIA.

The word *Anaemia*, like that of Plethora, corresponds to a certain number of badly-defined morbid states, which are all interpreted, in a hematological point of view, as a diminution of red globules, and which M. Bouillaud has distinguished in a nosological manner into *Anaemia*, *Hydraemia*, and *Chlorosis*. Hematology takes all the analogous affections in one body, from the *anaemia* of men who have been long under the influence of lead poisoning, that of miners, that of individuals whose constitution is seriously affected by a chronic disease, idiopathic *anaemia*, consecutive or secondary *anaemia*, *chlorosis*, and similar affections; and it finds in all these states an identical alteration of the blood.

M. Andral found, as a mean of the number of the globules in sixteen instances of incipient *anaemia*, the number 109; and in twenty-four instances of confirmed anaemia, the number 65. These were instances of spontaneous anaemia. The lowest number in the globules was in these cases 28.

MM. Becquerel and Rodier, in thirty-five analyses of blood in cases of anaemia, proceeding from very different causes, found the following results.

	Mean.
Density of defibrinated blood,	1047·4
" of serum,	1017·1
Water,	822·
Globules,	94·7
Albumen,	68·
Fibrin,	3·5
Extractive matters and free salts,	8·
Fat matters,	1·806
Seroline,	variable.
Phosphuretted matter,	0·663
Cholesterine,	0·110
Soap,	0·992

In 1000 grammes of coloured blood there were—

Chloride of sodium,	3·5
Soluble salts,	2·4
Phosphates,	0·545
Iron,	0·366

These results are interpreted in the following manner by the observers by whom they were obtained.

1st, Considerable diminution in the density of the defibrinated blood, which is explained by the diminution of the globules. *2d*, Maintenance of the density of the serum, owing to this circumstance, that the albumen is not sensibly diminished. *3d*, Very remarkable diminution of the proportion of the globules. *4th*, Slight augmentation of the fibrin (3·5 instead of 2·2). *5th*, Slight augmentation of the amount of fat substances; augmentation of fat phosphuretted matter; no change in the cholesterine; diminution of the animal soap; no change in the chloride and soluble salts; slight augmentation in the phosphate of lime; diminution of iron along with that of the red globules.

This complete analysis confirms all the results obtained by M. Andral.

The authors deduce the following conclusion.

The diminution in the proportion of red globules characterizing the general state to which the name of Anaemia is given, is frequently observed in diseases, either as an essential character, or as a complication, or as a consecutive phenomenon.

This is a mere outline sketch. Hematology has yet a large field to examine, if it be wished, perfect analytical processes, to distinguish these different sorts of Anaemia from each other. Practical Medicine, which sometimes confounds them, as often distinguishes them by particular symptoms. Hematology upon this question is still behind practice.

MM. Andral and Gavarret, have shewn that in Chlorosis the numerical proportion of the fibrin, instead of being diminished, is even comparatively increased in reference to the globules; and MM. Becquerel and Rodier subsequently shewed that the quantity of fibrin is absolutely augmented in this affection in particular.

What is the conclusion to be deduced from these facts, except that this state of impoverishment of the blood demands in all circumstances a treatment almost identical. This is the recommendation of Practical Medicine, which requires in this class of cases the use of tonics, chalybeates, and especially animal diet.

Regarding the etiological inferences deducible from the diminished amount of iron in the blood, I know no fact which proves that the iron of chlorotic blood is not diminished in proportion to the red globules. The greater part of the iron in the blood is furnished by the red globules; and in Chlorosis we observe the proportion of iron fall and rise with that of the globules in the state of disease, and according to the effects of treatment.

ARTICLE FIFTH.

ON THE BLOOD IN PLETHORA.

There are in medicine many morbid states, upon the true nature of which we have very imperfect facts and vague inferences. In the number of these states may be placed *Plethora;* and in order to define it, pathologists were obliged to say that in it the blood is abundant and rich. Meanwhile, previous to 1840, an opinion more precise prevailed; this was, that the blood, the organic elements of which had become more abundant, was in particular more rich in fibrin. This opinion, however, was founded upon no strict analytical examination.

The researches of M. Andral have shewn that it is not true that in Plethora the blood contains remarkably more fibrin than in the physiological state. Thirty-one bloodlettings performed upon very plethoric individuals gave a mean in 2·7 of fibrin. Among these individuals the fibrin reached not even the physiological number. [The author should have said the maximum physiological number, 3·5. The mean is 2·2.]

The organic materials of the serum present not in Plethora any remarkable changes in proposition.

It is upon the numerical proportion of the globules that turns the change which distinguishes the blood in plethora. In thirty-one bloodlettings, M. Andral found the mean number of the globules to be 141 per thousand parts; the minimum being 131, and the maximum 141.

[Some confusion appears here in the reasonings and deductions of the author. Under the head of the Analyses of Normal Blood, the globules are stated, according to the analyses of MM. Prevost and Dumas, Lecanu, Andral, and Gavarret, to be represented by the number 127 in 1000 parts. According to the analysis of MM. Becquerel and Rodier, which is believed to be a rectified one, the globules are stated at 152 for maximum, 131 for minimum, and 141·1 for mean. This, it is to be remembered, is still normal blood; that is to say, the globules may be so low as 131, and so high as 152 per thousand parts, without going beyond the limits of health. The author states here, however, that it is in the numerical proportion, the cypher, as he calls it, of the globules that the change in the state of the blood in plethora consists. He afterwards states this to be 141. This is, in point of fact, the normal or healthy number. It is manifest that if this is correct, Plethora differs not from the state of health as to the proportion of globules; and consequently these estimates are of no service, or they are erroneous. The modern chemical Pathologists, consequently, have not elucidated the Pathology of Plethora any more than the ancient mechanical Physicians.]

The diminution of the water is in proportion to this augmentation of the numerical amount of the globules.

From these analyses it results that the blood of plethoric persons differs from ordinary blood by the greater quantity of globules, and by the smaller quantity of water which it contains.

Some years subsequently, namely in 1844, MM. Becquerel and Rodier deduce, from their analyses of blood in instances of plethora, the following inference. "The plethoric state, and the accidents which accompany it, arise probably from the augmentation of the quantity of blood normally contained in the organism, but by no means from any change which has taken place in the composition of this fluid, and in particular from any increase in the proportion of the globules.

It is proper to add, that these observers, after an analysis of the blood in eleven individuals, who were in a satisfactory state of health, thought that they were entitled to establish the inference, that the number of the globules in 1000 parts is superior to that of 127, generally received as expressing the physiological mean, (Lecanu, Dumas), and that it varies in the healthy human subject between 152 and 131; mean, 141.

This discordance in the results demonstrates the necessity of paying attention, in analyses of normal and altered blood, to differences induced in the composition of the blood by age, by sex, and perhaps by other circumstances; for example, the last ingestion of food and drink, and the time which has elapsed since that ingestion; also temperature.

As to the suggestion offered by MM. Becquerel and Rodier, that "probably the blood normally contained in the vessels is augmented," I cannot deduce this inference from a negative result in the analyses of the blood, any more than I can conclude, from the analyses of MM. Andral and Gavarret, that the quantity of blood is not increased because the globules are in excess. The fact of increase or of diminution of the total mass of blood is one of the problems of Hematology least capable of solution; and it is important not to go too fast in a subject so obscure, especially when our object is to shew actual knowledge in this branch of science.

Whether the blood is rich in globules or in other principles, or its mass is increased, the first practical indication in

Plethora, in accordance with hematological principles, is to withdraw from the system a quantity of blood such that the vessels may be partly emptied, and that the entire mass of blood may be diluted by the absorption of a larger quantity of water.

ARTICLE SIXTH.

ON THE BLOOD IN HEMORRHAGIES.

I shall treat separately of the blood in Scurvy and in Purpura. I shall restrict myself to remark, that of the two conditions primarily established by M. Andral as favouring the production of Hemorrhagies,—namely, on one hand, an excess of globules, and a diminution scarcely perceptible of Fibrin; on the other hand, a diminution of Fibrin and a normal quantity, or a diminution of globules,—the latter condition cannot be admitted at the present time; the first is to be referred to the head of Plethora, and requires no farther consideration, since the conditions which there facilitate hemorrhagy may also depend on mechanical causes, such as the impulse of the heart, or the particular condition of the blood.

ARTICLE SEVENTH.

ON THE BLOOD IN DROPSIES.

It was almost recognised previous to 1840, that certain dropsies took their origin in the blood; but they were ascribed to an impoverishment of this fluid, without defining the nature of the alteration. If we remember that there are forms of *anaemia*, viz. *chlorosis*, in which the aqueous part of the blood has undergone a considerable increase at the expense of the red globules, and in which we meet only exceptionally with dropsies, we shall be very reserved in the admission of this sort of cause. M. Andral has only very rarely seen serous effusions ensue, even in cases in which the considerable diminution of the blood-globules had taken place in an instantaneous manner. It may therefore be said almost *a priori*, upon reading the Essay on Hematology, that if there were any state of the blood which ought to favour the production of dropsies, that state might be distinguished by the diminution of the albumen of the blood. Already Bostock, Gregory, Christison, had shewn that the albumen of the blood is diminished in the disease of the kidney described by Dr Bright.

I shall not discuss the question whether in dropsies of the kind now specified the diminution of the albumen is idiopathic, or whether it do not depend on the passage of the albumen into the urine, in consequence of granular *Nephritis*, or other causes which act in the same manner. Science was not decided in this respect, for want of conclusive observations, when MM. Becquerel and Rodier, in 1850, by studying a certain number of morbid states, independent of any Albuminuria came to form the following conclusions.

1. In the same manner as there exists an *Anaemia*, from diminution of the proportion of blood-globules, we ought equally to admit a peculiar pathological state, distinguished by lowering of the number of albumina in the serum.
2. A great number of acute dropsies considered at the present time as idiopathic, ought manifestly to be ascribed to this pathogenetic cause.
3. The diminution in the proportion of the albumen of the blood is completely independent of the numerical amount of the globules. These two alterations of the blood nevertheless very often exist together.
4. The diminution in the proportion of the globules is entirely incapable of producing dropsy.
5. The causes capable of producing the slow and chronic diminution of the albumen of the blood, are insufficient aliment, considerable losses of blood, diarrhoea long continued, marsh poisoning.[1]

If these facts are confirmed, MM. Becquerel and Rodier shall have rendered an actual service to the doctrine of Hematology, by inscribing in the Nosological list a new variety of *Anaemia.*

Is there, as M. Blot thinks, any relation between the Albuminuria of pregnant females, and uterine hemorrhagy after delivery, in the same manner as there is between Albuminuria and *Eklampsia*, or Convulsions ? This is a question to be resolved, which is naturally placed after the preceding problem.

ARTICLE EIGHTH.

ON THE BLOOD IN SOME DISEASES COMMONLY CALLED ORGANIC.

This Article comprehends Tubercles, Cancer, Hypertrophy. Other affections might be placed in it; but we possess so few exact analyses, and it is on the other hand so difficult to keep an account of the multiplied influences which tend to complicate the results, and render them variable, that I think it not useful to dwell long upon this subject. I shall merely say, in the way of practical application, that, if we know no substance capable of restoring to its natural state the altered fibrin; if we change, by bloodletting and the other antiphlogistic means, the state of the blood in instances of local inflammation, in scrofulous, chlorotic, and cachectic conditions, in which there is in the blood an excess of badly elaborated fibrin, the indication is to nourish and to give tone. Possibly in these instances Alkalies and Iodide of Potassium act by dissolving the fibrin, and preventing its deposition. But in all the instances, it is further necessary to expect from dietetic remedies, nutritious regimen, pure air, suitable exercise, and similar agents, important influence in the treatment of these maladies.

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[1] These, 1849, Paris.

After having examined the blood in these different morbid tribes, we have to consider it in some special diseases, in which have been remarked alterations in its composition, which derive some interest from a detailed examination.

(To be concluded in next Number.)

Art. VI.—*Some Observations on the Effects of Cholagogue Medicines, and some Remarks on Morbid Changes in the Liver. Fatty Liver; Lardaceous Liver; Cirrhosis; Jaundice.* By C. Handfield Jones, M.B. Cantab., F.R.S., Assistant Physician to St Mary's Hospital. Communicated by H. Bence Jones, M.D. Cantab., F.R.S., Physician to St George's Hospital. (From the Medico-Chirurgical Transactions, Volume Thirty-fifth.)

1. A short summary of the author's views on the structure of the liver may properly precede the following account of the effects produced by certain cholagogue medicines, and some appended remarks on various morbid changes that are incident to the liver.[1]

The mammalian liver consists mainly of a parenchymatous solid mass, marked out by the portal canals and fissures into lobules, which blend at their margins with each other. In the three lower vertebrate classes, the lobular arrangement is scarcely to be observed, and is, therefore, evidently nonessential. The parenchyma consists of perfect cells, or nuclei, and diffused granular and oily matter. The ducts do not expand to inclose the parenchyma, nor are they prolonged as intercellular passages through it. They terminate either by closed rounded ends, or, as Kölliker believes, by abutting against the mass of parenchymal cells. The structure of the ultimate ducts is peculiar, consisting chiefly of nuclei, and seems fitted to exert an active elaborating function; they do not resemble mere efferent channels. The ducts are supplied by the hepatic artery throughout their larger branches; but the ultimate ones are surrounded only by a scanty plexus, as they lie in the interlobular fissures and are exposed besides to the secretion of the marginal cells. The portal vein is distributed exclusively to the parenchyma, the cells of which lie in the closest relation with its capillaries; the hepatic cells elaborate out of the blastema supplied by these capillaries, the sugar which is formed in the liver during digestion, and which is again absorbed and carried off by the blood passing out by the hepatic vein. Yellow matter (pigmentary of the bile) is often seen in the cells, especially in the central cells of human livers; it is not nearly so often seen in the marginal, in which oil is much more apt to collect. The yellow matter may be extracted in large quantities by alcohol, and gives the reactions of biliary pigment, but not those of choleic acid and its conjugates. The author believes that these are formed out of oily, saccharine, and albuminous matters by the ultimate ducts, the nuclei of which remaining permanent, exercise constantly a "metabolic" action.

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[1] [The paper on the structure of the liver by Dr C. Handfield Jones is published in the volume of the Philosophical Transactions for 1849, volume 139, page 109. Of this paper it was our intention to give some account, but other matters have hitherto concurred to prevent us. It may, in the meantime, be recommended as containing the most correct account hitherto given of the minute microscopical anatomy of the liver in different classes of animals. A previous paper by Dr Jones is published entire in the Seventy-fifth Volume of this Journal, page 114, for July 1850.]

When the development of the liver is traced in the chick, the parenchyma is found to appear first; soon after, an eminence, consisting of dark oily matter, is seen on the wall of the future duodenum, from which two offsets proceed to the parenchyma of the liver. They do not, however, ramify in it, but retrograde and waste; the eminence, however, remaining. The cystic and hepatic ducts begin to be developed at a somewhat later period close to the liver, extend downwards, and open together into the intestine, just exactly at the eminence mentioned. For this eminence, the author proposes the name of *colliculus*. The process in fishes and reptiles is essentially similar; in the former it proceeds very slowly; the liver is long a mere parenchymal mass, with a vesicular gall-bladder attached. The hepato-cystic duct in tadpoles is lined at one time by a ciliated epithelium.

2. I now proceed to detail some experiments which I have

made relative to the action of cholagogue medicines, in which a careful examination was made of the intimate condition of the different organs wherever it appears desirable.

To a healthy cat I gave Hydr. c. Cretâ, gr. v; she was killed by a blow on the head. The liver was found extremely gorged with blood, the gall-bladder containing a very dark yellow bile. The bile contained no visible object but columnar epithelium; its colour was unlike that of the yellow-tinted cells often seen—more of a reddish-yellow. I examined several thin sections; the capillaries were congested most excessively with blood, and formed a uniform plexus, with circular or elongated meshes. The diameter of the capillaries varied from $\frac{1}{2500}$th to $\frac{1}{4000}$th of an inch. The cells everywhere were pale, well-formed bodies, with soft mottled contents, imbedding numerous small oil drops. The envelope and the nucleus were pretty distinct. None exhibited the least yellow tint,—neither the marginal ones nor those lying near the centres of the lobules. There was no trace of bile in the small intestine. I injected afterwards warm water into the hepatic vein, and washed out the blood; after this I made a watery extract of the part thus freed from its blood, and easily obtained evidence of abundance of sugar being present. This could only have existed in the cells. In this case, the dose of the mercurial produced no evident effect, except that of causing considerable congestion.

I then tried the effect of larger and repeated doses. Three doses of calomel, gr. iij. were given to a full-grown cat, at intervals of about twelve hours:—she was killed fourteen hours after the last. The liver was extremely loaded with blood; the gall-bladder, as well as the ducts, contained much deep yellow bile; there was a faint tinge of bile in the middle of the small intestine, which increased towards the lower part, and became very marked there and in the large intestine. On examining thin sections, it was apparent that a prodigious quantity of yellow material had been formed in the cells of the margins of the lobules, and in those extending about half the distance to the centre. It appeared as yellow-coloured oil drops, which were quite distinctly *in* the cells; some few larger masses of reddish-brown or yellow aspect were free. On dissecting out the ducts nothing different from their ordinary condition was observed. The nuclei were not tinged yellow. The lungs contained numerous whitish masses of varying size, consisting of well-formed granular cells, nuclei, &c.; they were not of the nature of tubercle, but rather of exudation. In this instance I am satisfied that the quantity of yellow matter seen in the exterior cells was the result of the calomel that had been given; some bile was also poured out into the intestine.

Another cat, for the sake of confirmation, was dosed in a similar manner. No bile was found in the small or large intestine; the gall-bladder and the ducts were full of dark-coloured secretion, as in the former cases. The cells of the parenchyma were found exceedingly loaded with oil; in some parts large masses of big oil drops were seen, or huge single oil drops. The cells about the centres of the lobules contained distinct yellow matter, either in the form of granules or molecules, or as a diffused fluid. There were seen here and there largish, reddish, or reddish-yellow, or brown masses, of a somewhat angular form, which were probably pigment matter. The smaller ducts were found perfectly natural; the larger ones and trunks often stained of a decided yellow colour. The lungs were healthy.

I am inclined to think that the enormous accumulation of oil in the liver was partly the effect of the mercurial as well as the accumulation of yellow matter in the cells, which, it is interesting to notice, was deposited not only in the cells but also *free* in their intervals.

To a kitten two or three weeks old I gave Hyd. c. Cretâ, about gr. vij. and killed her four hours after. There was a good deal of mucus in the intestinal canal, and the gall-bladder contained much dark yellow bile. The lobules of the liver on their margins, where they bound the fissures and spaces, were decidedly tinted with yellow matter, which contrasted remarkably with the pale, granular, and oily condition of the larger remaining parts. The cells were usually pale, granulous particles, well and definitely formed; they contained some oil drops, and were imbedded in oily matter,—but this was less abundant than usual: the yellow matter was seated in the cells. On dissecting out the ducts, yellow-tinted cells and bright deep-yellow particles were seen adhering to the Glissonian sheaths; none were seen in the ducts. The ultimate branches of these, as is usual in young creatures, were not completely formed,—not of a definite shape; their nuclei were more widely separated than in the adult, and less isolated from the sheath itself.

I will also quote an earlier observation which I made upon a large, rough greyhound dog. He had Calomel, gr. v. given him, and was killed sixteen hours afterwards. The gall-bladder was full of healthy bile. The liver was much congested with blood: the cells were of a pale granular aspect, well defined, containing generally a few yellow molecules. Those situated on the margins were especially the seat of the yellow matter; it was seen in this situation constituting yellow sheaths or patches. The arrangement of the cells was plexiform rather than linear; there was but little oily matter in them. The structure of the ultimate ducts was quite natural; but among the nuclei numerous yellow molecules were seen clustering, as it were, over them; these were very similar to those in the hepatic cells.

This observation, as well as the preceding, leave no doubt on my mind that the action of mercurials on the liver is to produce a greater quantity of yellow matter in the cells. While I regarded yellow matter as identical with bile, relying solely on the indication of colour, I felt no doubt that this was biliary matter, and supposed that these experiments afforded even more positive proof than we had before, that the action of mercurials increases the production of bile; that is to say, that it not merely increases the *flow* of bile, by emulging the ducts and gall-bladder of their contents, but actually causes a greater quantity to be formed. And indeed, it may still be so; the quantity of glycocholate of soda, the organic salt of the bile, may really be increased; but of this the above experiments do not give any proof. It will, however, be interesting to compare the above results with those obtained when other cholagogue medicines were given, some of which influenced the *flow* of bile much more decidedly than mercury.

Colchicum.—To a healthy young dog a rather large dose of the wine was given. It produced vomiting and several clay-coloured evacuations. The liver presented hepatic venous congestion of first degree very well marked; the gall-bladder was greatly distended with greenish-yellow bile. The stomach was empty; its mucous membrane was of a pale white, with a blush of red here and there; its secretion faintly acid. The duodenum was empty; its surface covered with a quantity of watery and mucous fluid deeply bile-tinged; its capillary plexus was injected here and there. The small intestine was covered on its inner surface in the upper half of its extent by a layer of deeply yellow-tinged mucous fluid; the lower half, or rather more, was paler, not covered with fluid, and without any yellow tinge; the limit to which the bile-flow had reached was very marked. The last two inches of the small intestine, as well as the caecum and large intestine, was covered by a red grumous fluid, which consisted of a tenacious, transparent, partly granular plasma, containing multitudes of mucous or puriform globules and blood discs. The mucous membrane covered by this red fluid was in parts injected and softened. In a section it was seen that there was thickening and granulous deposit around the Lieberkühn tubes, and some enlargement perhaps of the solitary glands. In the liver the capillaries surrounding the intra-lobular vein, and the interlobular veins also, were congested with blood. The cells were pale granular bodies, without any yellow tinge; they lay in a granulous basis matter, which also contained free nuclei and small groups of free oil drops. In some few cells here and there were seen bright yellow particles extremely distinct; but none such existed in the vast majority. The ducts were in that opaque condition so common in London dogs, and which renders their course so easy to trace. This depends on the deposit of numerous oily molecules between the nuclei; it is not the minute ducts only that are affected, but the whole extent of the excretory apparatus. The columnar particles of the gall-bladder and larger ducts are filled in their interior with minute oil drops, and the surface in the fresh state is of a dead white aspect.

The effect of colchicum thus appears to be rather that of emulging the excretory passages of bile, than of promoting the formation of yellow matter (perhaps biliary) in the cells.

A cat was dosed twice with half a grain of tartarized antimony: she died of the effects. The liver was rather congested; the gall-bladder was full of bright yellow bile, which was very tenacious, and contained much visible oily matter. Bile had flowed out plentifully into the intestine. The cells of the hepatic parenchyma were very perfect, and contained, especially on the margins of the lobules, much oily matter; they were in no part tinged yellow. The ducts were quite natural.

Thus, there was evidence of bile having been emulged by this drug from the excretory passages, but none to shew that an increased quantity had been *formed.* The effect produced on the mucous surfaces of the intestine and pulmonary cavities, and on the blood in various parts, was very remarkable, and is detailed elsewhere. (*Vide* "Med. Times and Gazette," March 19, 1852.)

To a young dog Aloës, gr. iij, was given twice; once, fourteen hours, and again two hours before he was killed. A curious circumstance happened before life was extinct. Strong inspiratory movements were going on, and when one of the large veins was wounded, air in large quantity rushed in; immediately after this a loud systolic blowing murmur, audible at some distance from the body, commenced, and continued for some time:—it evidently was occasioned by the frothy condition of the blood expelled by the contraction of the ventricle. The liver was quite natural, moderately full of blood; the gall-bladder was moderately distended with yellowish bile. The whole length of the intestinal tract was tinged yellow by the bile which had flowed out; the mucous membrane was nowhere injected, or shewed a trace of irritation; the epithelium remained attached. There was accumulation of oily matter in the central cells of the lobules of the liver; it constituted distinct whitish spots about the size of a pin head. The cells were generally pale, granular bodies, and did not contain more than a very little oil, nor had they any decided yellow tint. The ducts were very distinct, being rendered opaque by oily deposit between nuclei.

Aloës in this case certainly acted as an efficient cholagogue, but no yellow matter was produced in the hepatic cells.

Oil of turpentine caused some bile flow, and distension of the gall-bladder with bile, in a dog; no yellow matter was observed in the cells. The lungs were a good deal congested, and in some parts hepatized. The intestinal mucous surface shewed traces of irritation; it was injected in parts, especially the large intestine, and covered with more or less of tenacious mucus.

Two doses of rhubarb were given to a kitten, at an interval of about twelve hours; two evacuations followed, the last a copious, solid, dark-green one. The liver was pale, the gall-bladder contained some bile, of a very dark, reddish-yellow tint; the cells were pale, well-formed, containing little oil and no yellow matter; the ducts were slightly opaque, from oily deposit. The whole of the mucous surface of the small intestine was decidedly but not deeply yellow-tinged,—that of the large intestine was not, it was pale and white.

Another kitten, of the same age, was treated with the nitro-muriatic acid bath six times, for five minutes at a time, in the course of four days. The blood of the hepatic vein contained well-formed globules, which sometimes formed *rouleaux*, and a moderate number of white corpuscles. The blood of the portal vein presented no visible difference, except that it contained numerous small corpuscles, the germs, probably, of new blood-globules. The liver was extremely pale,—the edges translucent; the bile in the gall-bladder of a very light yellow colour, altogether unlike that of the preceding instance. The cells were perfectly pale, devoid of any yellow tint, and contained very little oil; there was scarce any free oil; the ducts were quite natural. The capillary plexus was most beautifully seen, and the cells lying in its interstices; the meshes were oval or circular, and exactly moulded on the cell, so as to leave no apparent intercellular passages. The small intestine was pale, or but very feebly bile-tinged in its upper three-fourths, in its lower part it contained a good deal of deeply bile-tinged matter; the large intestine was full of ordinary dark-green fæces. The stomach was quite healthy, it contained much half-digested food, and gave a distinct acid reaction. The bile-tinged matter in the lower part of the small intestine was chiefly mucus; in this some long muscular fibres were entangled, which were very particularly bile-tinged, very much more than the surrounding mucus.

An adult cat, similarly treated, presented more marked traces of bile-flow, but the bile in the gall-bladder was of the ordinary dark-green colour; the hepatic cells contained numerous concrete, oily molecules, and were devoid of any yellow tint. Thus nitro-muriatic acid does not increase the production of yellow matter in the hepatic cells, but rather seems to tend to diminish it; it also seems to have a cholagogue effect.

A full-grown cat took, during three days, eight grains of taraxacum (the extract) daily, and then for two days longer, twelve grains daily. On examination, the liver was found perfectly natural, the gall-bladder contained bile of the usual dark-green colour. The stomach was empty and pale, except near the pylorus, where it contained some deeply bile-tinged mucous fluid; this extended to the adjacent part of the duodenum, but below this the colour diminished, and the whole surface of the small intestine, except the lower fifth, was barely tinged of a light yellow colour. The lowest part of the ileum was of a dull white, manifestly different from the above, but contained here and there some fæcal matter, with deeply yellow-coloured mucus; the surface of the large intestine was decidedly tinged yellow, and the canal was full of natural fæces. There was considerable accumulation of oily matter in the central parts of the lobules of the liver; the cells in the external parts contained a moderate quantity of oil, and were surrounded also by much free oily matter. No yellow matter was visible in them. The ducts, on being dissected out, were found quite natural.

A cat, pregnant with four fœtuses, which were at about the end of the first third of intra-uterine existence, was dosed with muriate of manganese. Half a drachm dissolved in water was given at mid-day, and again in the evening; she was found dead in the morning. She had not been purged. The lungs and heart were healthy, the former crepitant but rather congested. The stomach was empty, its mucous membrane of a rather dark red; there was no trace of yellow matter throughout the whole length of the intestine, small and large; the only contents were mucus, and some remains of food. The liver was rather full of blood, the gall-bladder very pale, containing very little bile, which was of an orange colour, and mingled with a good deal of mucus; under the microscope it shewed nothing but columnar epithelium, a little oily and granular matter. The cells of the liver were very well formed, generally more opaque than usual, many of them contained yellow matter, so that when *in situ*, streaks of yellow matter were seen radiating outwards towards the periphery: the yellow matter did not appear to be in one part more than in another. There were also some masses of free yellow matter. The ducts appeared natural. There was no yellow matter to be seen in the spleen.

The general result of the foregoing experiments on the action of the various cholagogue substances tried may be stated in the following manner. Mercury, muriate of manganese, and colchicum, are the only ones which seem to increase the production of yellow matter in the cells of the liver. I have placed them in the order of their potency. That they also increase the production of glycocholate and taurocholate of soda, I think very probable; but unless we were assured that the quantity of these principles is always proportionate to that of the yellow pigment, which does not seem to be the case, the above experiments say nothing as to this point. It is clear that the cholagogue action of a medicine, its emulging effect on the ducts, is distinct from that which it exerts in the *production* of biliary pigment. Jaundice has been known to come on during a mercurial course, and apparently an effect of the medicine. I have at present a patient under my observation (one of Dr Chambers in St Mary's Hospital), who reports her jaundice to have appeared after taking a six-grain calomel pill. These statements are in accordance with the experiments which I have detailed. The animals to which I gave mercury had jaundice of the liver.

One very important effect to be noticed of the administration of mercury on the liver is, that it certainly produces very great congestion of the organ. This is an argument for rather forbidding the use of this remedy in inflammation of the substance of the liver, a practice which would otherwise be recommended by our analogical experience.

3. The fatty condition of the liver, which is so often observed, has been so well described and examined, that it must appear superfluous to offer any remarks upon it. I think, however, there are some points of detail which are worth a short notice. It seems a fair question, What constitutes truly fatty degeneration of the liver? Is it to be regarded as a simple increase

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to this instance I will place the notes which
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liver was in a complete state of fatty degeneration;
drops, however, did not seem to be inclosed in distinct
but to lie in an indistinct granular or semi-fibrous stra-
; scarce any distinct cells were to be seen." In the second,
the liver being also in a state of complete fatty degeneration, *I observed* that the cells were, for the most part, destroyed, *and* that only films of granular matter remained; the number *of free* nuclei also seemed to be fewer than usual. In these *two* cases it was quite evident that the natural cell structure *of* the liver was, to a great extent, destroyed;—that a real degeneration had taken place; and this I have repeatedly observed in most cases of fatty degeneration of the liver, occurring in persons who have died of various diseases. Whether a liver the cells of which are thus destroyed can recover its natural condition, is a question of much interest, but one which it seems impossible to answer.

A circumstance of much interest, which seems still further to indicate the degenerative character of the fatty transformation of the human liver, is, that in such livers I have almost invariably been unable to detect the existence of sugar; and this is confirmed also by the experience of my friend Mr Blyth. This, however, was not the case with the liver of the kitten which had been fed on fatty food. It yielded on analysis a full proportion of saccharine matter.

Another point which has not been much noticed is the peculiar limitation of fatty degeneration to the margins of

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in a few here and there; the common condition of livers which present more or less of the "nutmeg" aspect, is oily accumulation or fatty degeneration of the margins, with heaping up of yellow molecules in the central cells around the intra-lobular vein.

Fatty degeneration of the liver, when complete, may occur in very different diseases; it is by no means peculiar to phthisis, as is well ascertained. Out of nineteen cases which I examined microscopically, four or five only had any connection with phthisis; the others occurred in diseases of various kinds, which, for the most part, but not invariably, had produced great emaciation.

4. Another kind of degenerative change is present in the much more rare *lardaceous* liver. One specimen of this kind which I examined, had the following characters: it was enlarged, bulky, of solid feel and glossy aspect, having a yellowish tint, with some whitish spots, and containing very little blood. Thin sections under the microscope shewed some of the lobules in a pretty natural state, the cells perhaps rather enlarged, and arranged very much in a linear manner. In other lobules there was a manifest infiltration between the cells of a homogeneous, highly refracting substance, much resembling concrete oily matter; this was very abundant in some parts, so much so as to compress the cells and reduce them to mere bands anastomosing together, and, producing the appearance of a plexus containing the homogeneous matter in its meshes. In many spots, especially in the vicinity of the deposited substance, the cells were deeply tinged with yellow matter. The refracting matter in another instance consisted of spherical, oval, or irregular-shaped

masses, which had somewhat the size and figure of the natural cells, but did not, I think, result from a transformation of these. Liquor Potassae added to thin sections under the microscope reduced the deposited matter to mere filmy fragments, and diminished considerably the refracting power; its action upon this substance and the surrounding cells, many of which contained much oil, was very different; the latter becoming more transparent, exhibited their oily contents with great distinctness; the former, though quite translucent, shewed no trace of oily matter. The minute ducts in the latter specimen, as well as in the former, were of natural appearance. I have several times examined spleens from the human subject, which contained a great quantity of matter precisely similar in appearance to that which was here deposited; its seat in the spleen appears to be chiefly the Malpighian bodies, whose natural cell-structure it replaces, and then extending outwards, brings about an atrophy of the red pulp, which occurs to such an extent, that at last mere red streaks are seen separating the translucent glistening substance, and the natural deep colour of the organ is lost. The term by which Rokitansky designates this peculiar deposit is the "lardaceous" (speckig), and the pathological relations he assigns to it is with "constitutional disease of the vegetative system, especially with scrofulous and rickety disease, with syphilitic and mercurial cachexia," and occasionally as a sequel of intermittent fever. Dr Budd recognizes this condition as a scrofulous enlargement of the liver; this I think is so far correct, as intimating its relation to scrofulous disease; but it ought clearly to be understood, that the deposit is totally different from tubercle, and is, indeed, so far as I know, *sui generis*.

The minute ducts in the fatty and last-described condition of liver have appeared to me pretty natural; this fact renders it more easy to understand how, with so considerable alteration of the parenchyma, bile can still be secreted.

5. Rokitansky, describing cirrhosis, says,—"We are not of opinion that granular liver always takes its origin in the same fundamental affection; we are inclined to adopt two morbid states as the essential and original anomalies which give rise to granulations in the hepatic parenchyma as a secondary affection. In one case there is a morbid development of the capillary gall-ducts, (the so-called secreting tissue), an accumulation of the secretion, and probably also an hypertrophy of the parietes of those vessels giving rise to the nutmeg liver, and to an obliteration of the capillary blood-vessels, the so-called vascular substance." . . . "In the

second case, the original affection of the hepatic parenchyma in granular liver is proved, by the post-mortem appearance of the granulations, to consist in a slow chronic inflammation. This induces a gradual obliteration of the parts attacked, and their conversion into fibro-cellular tissue, the amount of which varies in proportion as the processes of absorption or organization predominate in the inflammatory product. This secondary metamorphosis, from not occurring uniformly, results in a subdivision of the organ into larger and smaller scattered compartments, which present the characteristic rounded form of the granulations in the same ratio as they correspond to single hepatic lobules."

Dr Budd, in his chapter on Cirrhosis, describes it as "the consequence of adhesive inflammation in the areolar tissue about the small twigs of the portal vein, by which serum and coagulable lymph are poured out. The serous part of the effusion gets absorbed, and the fibrin contracts and becomes converted into dense fibrous tissue, which divides the lobular substance of the liver into well-defined masses," &c.

I cannot say, that I have observed any condition corresponding to the first form of cirrhosis, which Rokitansky describes. A morbid development of the capillary gall-ducts must mean a morbid enlargement of the hepatic cells of the lobules, and such I have never seen in this disease. Dr Budd's description expresses the opinion ordinarily received, to which I have but little to add that can be esteemed new. I think, however, that a somewhat different view may be taken of the nature of the morbid change constituting cirrhosis, and that some of its different forms may be more exactly traced.

In some cases, no doubt, the fibrous tissue of the Glissonian sheath is affected with what may be truly called adhesive inflammation, attended with the effusion of coagulable lymph and serum. These cases will be marked by tolerably distinct symptoms;—pain in the side, vomiting, fever, perhaps jaundice, as Dr Budd enumerates them. The portal canals are the seat of the inflammation,—chiefly, I believe, the larger, and not to any great degree the smaller, canals or the fissures. In other instances, which I believe, are more numerous, the same parts may be affected, but the nature of the change is different. The fibrous tissue of the sheaths undergoes thickening and condensation, but not as a consequence of effusion of lymph and serum in an acute attack; the process is much more of the nature of degeneration, more akin to the chronic thickenings which are so often observed in fibrous-serous membranes, and which are often coincident with this condition of the liver. The result of the above changes, when they do not extend to the smallest portal canals and interlobular fissures, is, I believe, the common hobnail condition of the liver. If the thickening and condensation proceed further, and affect the sheaths which surround and invest the lobules, then the condition is different, and corresponds very nearly with that which I described in a communication to the Pathological Society, three years ago, under the name of "nutmeg liver."

This description I subjoin; but remark at the same time, that in several of these cases I have no doubt, that there takes place also in the substance of the lobules, a deposition of unhealthy plasmatic fluid, which solidifies them, and tends to increase still further the atrophy of the cells, which the defective supply of blood induces. It is difficult in examining the *débris* of the parts thus degenerated, to distinguish accurately, in all particulars, between what should be regarded as cause, and what as effect; but, though aware of this, I still am strongly inclined to believe that such a process as I have indicated does actually affect the lobules themselves, and that the unhealthy nutritive process, which is the essence of cirrhosis, may manifest itself chiefly in either of three situations:—1. In the larger and middling-sized portal canals, excluding only the smallest; 2. In these last, and in the fissures; 3. In the smaller canals and fissures, and in the substance of the lobules. The first form results, as said, in the common hobnail liver; the second and third produce tough, firm, dense livers, which are sometimes termed "brawny."

The following is the quotation from the Report of the Pathological Society.—"The form of the liver, in the condition referred to, is scarcely at all altered; its edges are not rounded, nor its surface puckered,—or, if these changes have occurred, they are but slight; its capsule is often smooth, and free from false membrane or other traces of chronic inflammation; its consistence, however, is greatly increased, it is much less fragile, and tears much less easily than natural. On the surface of a section, deep red patches of sanguine congestion are observed coalescing irregularly with each other, and leaving in their interspaces pale grayish or slightly yellow spots; the relative size of the congested and non-congested patches may vary somewhat, but the boundary between them is extremely well-defined, the deep tint of red ceasing abruptly, and not shading off into the pale hue of the circumscribed gray spots. Under the microscope it is seen that the cells in the central parts of the lobules, which are the chief seat of congestion, are remarkably altered; they are gorged with yellow matter, and appear as round or oval masses of dark yellow or reddish substance; they are much less numerous than in the healthy state, and lie no longer closely in contact with each other. This condition of the secreting structure is co-extensive with the congestion of blood; beyond this, towards the exterior of the lobules, a very different condition is found to exist; here the cells are no longer distinct from biliary engorgement; on the contrary, they are pale, stunted, starved, and often scarcely discernible, or appearing as mere *débris* in the midst of an amorpho-granular basis substance. In some cases the fissures are greatly enlarged: instead of having a diameter of $\frac{1}{833}$ inch, or rather less, I have found several to measure as much as $\frac{1}{54}$, or even $\frac{1}{44}$ inch; this increase in width taking place, of course, at the expense of the lobules, which thus become considerably diminished. The investing membrane of the lobules is very greatly increased, and becomes much more condensed and more distinctly fibrous; it has appeared to me in some instances to be continuous with the amorpho-granular substance which infiltrates the pale external portion of the globules. It also seems to me very probable that these cases, where the fissures become so greatly enlarged at the expense of the parenchyma, are but further stages of the degenerating process above described; for, as the exterior part of the lobules becomes atrophied from the deposition of an amorpho-granular basis substance, and loses its natural structure, it will tend to become blended with the fibrous tissue of the continually enlarging fissure. The foramina, which are commonly so evident in thin sections existing at the interlobular spaces, are no longer apparent, the pressure of the thickened fibrous tissue having obliterated the corresponding branches of the portal vein.

" The essential circumstance in the changes now described seem to be the effusion of an unhealthy plasma, not only in the canals and fissures, where it induces unnatural increase and condensation of the fibrous sheaths, but also in the external part of the lobules, where it passes into a solid form, and constitutes an amorpho-granular basis substance, compressing the capillaries and stunting the secreting cells. At the same time the thickened fibrous tissue of the canals and fissures still further obstructs the arrival of blood in the lobules, which thus at last are scarcely supplied otherwise than by the reflux of blood in the ultimate twigs of the hepatic vein and the capillaries which immediately surround them; and thus the morbid congestion of the interior of the coincident with this condition of the liver. [illegible] engorgement [illegible] the above changes, when they do not extend [illegible] on their portal canals and interlobular fissures, is, [illegible] the liver had mon [illegible] condition of the liver. If [illegible] of the mitral condensation proceed further, and aff[illegible] the tissue of surround and invest the lobules, th[illegible] confirms the view [illegible] and corresponds very near [illegible] nature of this [illegible] in a communication to the [illegible] generation of the se[illegible] years ago under the name of " [illegible] of the plasma sup[illegible]

This description I subjoin [illegible] inflammatory action pro[illegible] that in several of these cases [illegible] as I have observed, this place also in the substance [illegible] in the irregularly [illegible] plasmatic [illegible] the result of cir[illegible] increase still further [illegible] so serious and [illegible] supply of [illegible] the [illegible] of the [illegible] thickening *and* [illegible] is not so much as cause, and [illegible] a slow degenera[illegible] an [illegible] valves of the heart I have [illegible] supported by the [illegible] Table. It con[illegible] appeared on micro[illegible] of the *fibrous* [illegible] In 13 of these [illegible] degeneration *taking* [illegible] may be considered [illegible] coincident change; [illegible] among the 14 [illegible] kidneys, with [illegible] This morbid [illegible] state of system [illegible] the fibrous tissue [illegible] by others. [illegible] *interesting,* [illegible] of tubercular [illegible] superseded by a [illegible] which might, [illegible] If, indeed, [illegible] ascertained by some [illegible] excrementitious or [illegible] one of the [illegible] of tissues,— [illegible] morbid *state as I* [illegible] higher degree [illegible] *as a form*

lethora, in which a fluid material, cha-
to spontaneous coagulation, and exist-
in undue quantity, produced inju-
rting the nutrition and spoiling
We should perceive a connec-
na which now appear isolated.
ons, so called, on the valves of the
rinous masses in the spleen or else-
r as instances of tumultuous and exces-
n in the blood, which was thus cast out
parts. The presence of fibrinous nodules
ule of the liver, or on the pulmonary pleura,
a process of a similar but much more gra-
All the various thickenings of fibrous or fibro-
embranes, stiffening of the valves of the heart,
tion of its orifices, shortening of its chordae tendineae,
y of the common pleural adhesions, white pericardial
tches would be properly ranked as having their origin in a similar morbid condition of the blood. They would appear then as manifestations of a general state, and not as local affections.[1] The common fibrous tumour of the uterus would be a particular localization of the general diathesis. There would appear reason to consider the large, firm, dense kidney, with its adherent capsule, as another result of such morbid action. The adhesion of the capsule to the surface is evidently the result of a similar change to that which thickens the capsule of the liver; and the increased bulk and coarser condition of the epithelium of the tubes may not improbably be attributed to its having undergone a kindred change, modified, of course, by its own peculiar nature.

I cannot help remarking that no term seems to me, in many cases, so exactly to describe the condition of the renal epithelium in this condition of the organ as the word *stiff;* comparing the morbid structure with the healthy, it gives one the idea of being as much incapacitated from undergoing its normal chemico-vital changes as a stiff and rigid aortic valve is from following its natural easy movement. Again, under the head of fibrinous disease, we should range various forms of so-called chronic pneumonia, in which with little appearance of acute inflammation, exudation into the cavities of the air-cells, and complete condensation of the tissue have taken place. Some of these cases, by the nature of the exuded material, would form a connecting link between chronic pneumonia (the fibrinous) and the recognized tubercular infiltration. The above are all instances of extra-vascular change produced by the fibrinous disease; one, however, we may refer to as originating in the interior of the vessels, and occasioning, by its arrest of the circulation, irremediable gangrene of the parts thus deprived of their supply of blood. I allude to the spontaneous coagulation of the blood within the vessels, of which several instances have lately been recorded, and which can be ascribed with no kind of probability to any other cause than such a state of the blood as we are now considering.[1]

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[1] Rokitansky says, "The bases for fibroid new formations are solid, most probably always fibrinous blastemata."

The preceding is rather a long discussion, but may be excused, I trust, from its intimate connection with the pathology of cirrhosis.

The form of cirrhosis which produces the "brawny" liver often co-exists with dilatation and hypertrophy of the heart, the former predominating more or less. The retardation of the circulation thus occasioned throws the blood back upon the large veins; and the hepatic veins in particular, with their radicles in the centre of the lobules, are in consequence much congested. I am strongly inclined to think that this congestion of the central part of the lobules has much to do with the peculiar alteration of the cells which there takes place, and which is almost exactly coterminous with the congestion. The bile-pigment is in all probability derived from the colouring matter of the blood, and it is worth noticing that this yellow matter is especially collected in those parts which are especially the seat of sanguine congestion. Hepatic venous congestion of the first degree is far the most common; and it is in the same part which this occupies, viz., in the centre of the lobules, that we find almost invariably yellow matter present in the cells.

Cirrhosis is sometimes combined with fatty transformation; a section then presents opaque inlets, the lobules gorged with fatty matter separated by wide, more transparent spaces, which are the enlarged fissures and spaces occupied by new-formed fibroid tissue.

The ultimate ducts in cirrhosis are of course involved in the dense fibroid tissue, and are more or less atrophied by its pressure; their nuclei appear stunted and starved, and the structure is sometimes altogether lost, at least the ducts can no longer be discovered. Usually, however, by the aid of acetic acid they can be seen pretty readily in the widened fissures and canals.

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[1] When the above was written I was quite unacquainted with Rokitansky's doctrine of fibrinous crases of the blood, contained in vol. i. of his work. The coincidence of opinion of independent observers is not without value.

6. Jaundice manifestly results from the conveyance into the blood of biliary pigment, that constituent of the bile which is certainly excrementitious and intended to be cast out with the fæcal matter. In most cases there is retention of the yellow matter in the liver, from a mechanical obstacle to its flow into the intestine, or from inaction of the excretory ducts; the retained matter, which, however, is not in excess, being absorbed into the blood instead of passing out with the secretion. In other cases it seems to be formed in excessive and unnatural quantity, as in the acute yellow atrophy of the liver, and perhaps in diseases of the heart, producing great congestion of the veins. It is extremely common to find yellow matter in the cells of the central parts of the lobules in cases of different description quite unattended with any jaundice. This is so frequently the case that it really seems probable that it results from the congestion of the intra-lobular veins and surrounding capillaries which takes place during the last hours of life. Great dilatation of the cavities of the heart, with consequent retardation of the circulation, produces most remarkable yellow engorgement of the central cells of the lobules; and this condition is, I think, not unfrequently accompanied with marked jaundice. In this case, however, I believe there is no cessation of the flow of bile into the intestine; the function of the ultimate ducts and of the exterior cells is still performed; and there seems only to be increased production of yellow pigment in the central cells. It must be borne in mind that this yellow pigment as it exists in the cells does not evidence the presence of biliary matter, of choleic acid, or its conjugates. The yellow matter can be extracted by alcohol, and gives the characteristic reaction of bile-pigment with nitric acid; but Pettenkoffer's test decides against the presence of the organic biliary acid. The deep colour of the urine in jaundice also seems to depend solely on the presence of the pigment; often no trace of choleic acid is discoverable. I conclude, therefore, that in most cases jaundice results from the absorption into the blood, not of completely formed bile, but of one of its constituents only, viz., the yellow pigment. This takes place; (1.) when a mechanical obstacle prevents the flow of bile into the intestine through the duct. com. choled.; the bile then accumulates in the larger ducts and distends them: it continues to be secreted for some time, but afterwards, accumulation of yellow matters takes place in the cells, from which it is no longer freely extracted by the elaborating ducts. (2.) When inaction

of the elaborating ducts occurs. This is the commonest case; the cells become filled with yellow matter, as before, because it is not withdrawn from them. (3.) When, with or without impairment of the action of the excretory ducts, an increased quantity of yellow pigment is formed in the parenchyma of the liver.

I may quote here some remarks from Prof. Lehmann's work on Physiological Chemistry, which seem to me not only to bear out the opinion he adopts, that the components of the bile are not formed in the blood, but also to support the view I have maintained, that the bile is not completely formed in and by the hepatic cells, but that this is effected by the action of the ultimate ducts. Lehmann says, "It is just in disease of the parenchyma of the liver that jaundice very seldom occurs; in fatty liver, bacony liver, and the rare tuberculosis of the liver, certainly never; even in granular liver and inflammation of its substance but seldom. Jaundice only presents itself constantly in diseases of the gall passages, and in acute yellow atrophy."

Now, if with these various alterations of the parenchyma of the liver bile can still be formed, as we know it often is, it seems highly probable that some other apparatus besides the cells has a large share in the formation of this secretion, or at least is capable of taking such.

I will conclude these very imperfect remarks on the subject of jaundice by the detail of two instances of acute degeneration of the liver, which I have recently had the opportunity of examining through the kindness of my friend Dr Ogle. The first is an instance of acute yellow atrophy, as it is termed by Rokitansky.

A. B—, a lad, admitted into St George's Hospital, Sept. 24th. He stated his illness to have lasted only one week, having had sickness and pain at epigastrium, and being jaundiced. His urine, however, had been darker than natural for a fortnight. He had taken balsam copaibae for gonorrhoea the last two months. On admission his pulse was 64, rather weak; bowels said to be relaxed, and motions of a light colour; had severe headache and pain of stomach. Fomentations: Hyd. c. Cretâ and Ol. Ricini. Motion next morning very offensive, of pale yellow colour; some tenderness in the right hypochondrium. Jaundice, in spite of Calomel, gr. j, ter. die, became considerably deeper, and he suffered from sickness. Bowels rather costive; motions very pale, but vomited matter coloured by bile. He began in about nine days to be mercurialised; pills omitted; but jaundice deepened. Nitro-muriatic acid was given. There was now no pain, but sickness very distressing; no food whatever retained. Latterly he became very drowsy; pupils dilated. On October 10th there was stupor, occasional delirium, and succeeding coma. He died on October 12th.

Post-mortem examination, 57 hours after death.—General jaundice; lungs and heart healthy; yellow serum in pericardium; endocardium tinged yellow; some yellow serum in peritoneum. Intestines healthy; dark olive-coloured fluid in duodenum and stomach. Liver weighed 2 lb.; the cut surface was of a bright gamboge colour, the ducts quite pervious. Gall-bladder contained much thinnish fluid, barely tinged by bile-pigment. Lymphatic glands of mesentery and about the lesser curvature of the stomach much enlarged; kidneys much congested, but healthy, weight 13 oz.; spleen healthy, weighed 6 oz.

Microscopic examination.—Liver flabby, apparently bloodless, of deep yellow colour. The parenchyma consists of a mass of broken-up granular matter, tinged deeply yellow and mingled with oil drops; here and there some large deep-yellow masses or globules are seen, which refract rather highly. Scarce a single cell, or even nucleus, is to be seen. Along the margins of the lobules and in the portal canals there is a considerable quantity of free oily matter. The addition of liquor potassae produces an abundant formation everywhere of small rod-shaped crystals, which tend to form crosslets; often, however, the limbs of the cross are more nearly parallel than at right angles. These are probably margarine or margaric acid. The ducts were dissected out; only one is seen terminating apparently by a closed extremity, which is pretty evenly rounded; it consists of obscurely visible nuclei, lying in a sub-granular basis, which also contains very many opaque rather large globules, resisting acetic acid, and whitish by direct light. Similar globules are strewed about in the canal or fissure, *i. e.*, in the surrounding material. A bloodvessel accompanying a large duct is covered with network of streaks, formed by rows of similar globules; these are spread over the vessel and in the neighbourhood, and also along the sides of a translucent canal accompanying a branch of the vessel; they are doubtless lymphatics. Some of the ducts are remarkably bulged at their sides, and infarcted; their nuclei are generally somewhat altered from their natural aspect, and the intervening substance is tinted yellow. This liver contained no sugar, by Trommer's test.

I regret very much that this specimen was not examined for cholic acid; it would have been very interesting to know, whether any of the special organic constituents of the bile could be discovered amid all the prodigious quantity of accumulated pigmentary matter. The alteration which the ducts had undergone was very remarkable, and shewed that their elaborating action must have been almost, if not entirely, arrested. The engorged state of the lymphatics seems to indicate that they had been taking upon themselves the function of the ducts, or endeavouring, at least, to do so. The globules, which rendered them so unwontedly distinct, consisted, I believed, of some concrete oily matter.

The second case was that of a person who died with diseased kidneys, peritonitis, and slight pericarditis, with diseased aortic valves. The urine had been albuminous, and pleurisy, with effusion, had occurred on the right side. There was no jaundice; the bile in the gall bladder was dark and thin; the capsule of the liver was thickened in patches. There was some old cretaceous tubercle in the upper lobe of the right lung. There were one or two white patches on the pericardium, which contained some fluid and shreds of lymph.

The liver was very soft and flabby, the capsule perhaps a little thickened; it was very full of blood. All the cells were more or less altered; some, I think the central ones, were mere masses of yellow matter, others were quite broken up into a granular detritus, mingled with very numerous oil drops. The *déperissement* of the cells was by far the most striking circumstance, and had certainly occasioned the lax and flabby state of the organ. The fibroid tissue was not materially increased. I could find no trace of the minute ducts, —they seemed to have perished.

This case cannot, I think, be regarded in any other light than as an instance of rapid degeneration taking place in the liver. The remote cause of this was probably the dyscrasia of the blood induced by the renal disease; the immediate cause must be mere matter of conjecture.

List of Cases in which more or less of thickening and condensation of the Glissonian sheaths or capsule of the liver was observed, and in which the presence or absence of similar changes in other parts is noted. The number of cases analyzed amounted to 74, out of these 30 were the subjects of the change in question.

CASE 2. Glissonian sheaths thickened, capsule of liver opaque. Valves of heart rather opaque, kidneys granular, fibrous tumour in uterus.

... 3. Liver firm and tough. Glissonian sheaths extremely dense and semi-fibrous, encroaching on the obules in some parts. Valves of heart (aortic) thickened, one adherent to side of aorta. Kidneys heavy, capsules adherent, texture firm. Tubercular cavities contracted.

CASE 5. Glissonian sheaths thickened, liver fatty. Uterine cancer.

... 8. Liver in an early stage of cirrhosis. Glissonian sheaths condensed, patches of thickening in the capsule. Renal capsules adherent.

... 11. Liver dense, Glissonian sheaths thickened. Mitral valve thickened. Kidneys remarkably dense and hard. White spot on pericardium. Apices of lungs puckered.

... 17. Capsule of liver considerably thickened in patches. Kidneys cysted, everted, granular.

... 18. Capsule of liver a good deal thickened in parts, and sprinkled over with minute fibrinous nodules. Kidneys infarcted, some of their inner tufts compressed. Old universal adhesions of right pleura.

... 19. Capsule of liver and Glissonian sheaths somewhat thickened. Kidneys small, dwindled, granular.

... 21. Liver fatty, with incipient cirrhosis. Kidneys large, infarcted, mottled. Vomicæ and tubercles in lungs.

... 22. Liver enlarged, surface puckered, Glissonian sheaths increased, encroaching on the lobules. Kidneys somewhat granular. Spleen firm.

... 24. Glissonian sheaths rather thickened. Kidneys infarcted with much oily matter. Capsules of spleen thickened.

... 27. Glissonian sheaths somewhat thickened, liver unusually dense. Kidneys dense in texture, rather shrunken, capsules adherent. Visceral pericardium rather opaque, and dotted over by fibrinous granules.

... 32. Liver unnaturally dense, slight increase of Glissonian fibroid tissue. Kidneys firm, tubes infarcted, pleurisy with false membranes.

... 40. Liver contained two or three fibrinous nodules, adhered to diaphragm. Kidney not healthy, capsule adherent. Tracheal, bronchial, and lumbar glands greatly enlarged; their substance was of an homogeneous granular aspect, not tuberculous, more like fibrinous matter. Lungs contained masses of crude tuberculous matter, yet not quite resembling ordinary tubercule; I should consider it of an intermediate nature between tubercule and plastic effusion. A white patch on the pericardium.

44. Glissonian sheaths thickened, capsule of liver thickened, its edges rounded. Kidneys shrunk, granular, cysted. Both pleuræ thickened, or covered by false membranes.

Pericardium thickened. Capsule of spleen considerably thickened. Parietal peritoneum thickened in parts.

CASE 45. Glissonian sheath rather thickened. Kidneys infarcted, tubes containing moulds.

... 47. Glissonian sheaths thickened, capsule puckered at one part. Aortic valves thickened. Chronic thickening of right pleura. Left pleura obliterated. Induration of pulmonary apices around tubercules, some masses of these inclosed in a fibroid cyst. Tricuspid valve a little thickened. Kidneys not healthy, containing cysts.

... 48. Glissonian sheath thickened, liver fatty. Kidneys rather infarcted, capsules somewhat adherent.

... 49. Liver rather large, its capsule considerably thickened. Glissonian sheaths of pericardial canals thickened. Kidneys coarse, tubes infarcted, capsules unduly adherent. Capsules of spleen thickened. Chronic thickening of pericardium. Peritoneum thickened generally. Mitral and tricuspid valves thickened in spots.

... 51. Liver nutmeg, fibroid tissue of pericardial canals thickened. Valves a little thickened. Kidneys large, coarse, capsules unduly adherent.

... 54. Liver nutmeg, fibroid tissue in portal canals and fissures thickened and condensed. Kidneys coarse, tubes infarcted. Mitral and tricuspid valves a little thickened. A little subpleural fibrinous exudation with condensation around a small cretaceous mass.

... 55. Glissonian sheaths thickened. Kidneys coarse, cortical tubes infarcted.

... 57. Glissonian sheaths greatly thickened, encroaching on lobules, which themselves seem to be invaded by a solidified substance. Kidneys certainly degenerating, capsules very adherent. Pericardial white patch, some recent pericarditis. Mitral orifice greatly contracted. Aortic and tricuspid valves slightly thickened. Capsule of spleen somewhat thickened.

... 58. Glissonian sheaths rather thickened. Died of scarlatinal dropsy.

... 59. Liver covered with old and recent false membranes. Glissonian sheaths thickened in parts. Lobules seem penetrated by a fibrous basis substance. Kidneys not healthy, structure confused. Right pleura obliterated. Slight recent peritonitis.

... 60. Glissonian sheaths slightly thickened. Kidneys coarse, tubes infarcted.

... 67. Glissonian sheaths somewhat thickened and condensed. Aortic and mitral valves thickened, condensed, and stiffened; mitral orifice somewhat contracted. Fibrinous deposits in spleen and kidneys. A fibrous tumour in fundus of uterus. Pericardium adherent.

CASE 69. Glissonian sheaths much thickened, capsules thickened considerably in two or three patches. Kidneys coarse, large, surface markedly granular. Surface of lungs puckered from induration around tubercules. Mitral and tricuspid valves thickened. Spleen enlarged, firm, its capsule thickened.

... 73. Glissonian sheaths thickened, capsule slightly rough. Kidneys large, tubes infarcted.

... 74. Glissonian sheaths much thickened, capsule thickened by a white patch in one part, in another the surface drawn in deeply. Considerable chronic thickening of the arachnoid. Granular disease of the kidneys.

One important deduction which may be drawn from this Table is the very great frequency, almost constancy, with which thickening or condensation of the fibrous structures in the liver is associated with actual degeneration of the kidney, or some approach to it. Out of the 30 cases, 26 exhibit this association.

The following list contains a summary of the various diseased conditions, coincident with which fatty degeneration, partial or complete, occurred:—

1. Fatty degeneration complete; margins of lobules most affected. Faecal abcess of caecum; extreme emaciation.
2. Ditto incomplete. Phthsis; cavities; granular kidney; albuminuria.
3. Ditto far advanced. Ascites; false membranes covering peritoneum and liver; pneumonia.
4. Ditto partial, affecting margins of lobules. Fever, symptoms resembling those of delirium tremens; pneumonia; hepatization.
5. Ditto complete. Pneumonia; hepetization.
6. Ditto partial, margins affected. Sudden death; heart's tissue degenerated.
7. Ditto partial, marginal. Hypertrophied heart; granular kidney, containing some cysts.
8. Ditto partial, marginal. Fever; pneumonia; kidney degenerated.

9. Fatty degeneration partial, affecting exterior half of lobules. Kidney granular, cysted; heart's tissue in a state of fatty degeneration; blood fluid.

10. Ditto partial, marginal. Chronic empyema.

11. Ditto complete. Scrofulous disease of hip; extreme marasmus.

12. Ditto partial, slight marginal, rather oily accumulation. Menorrhagia; extreme anaemia.

13. Ditto partial, marginal. Fever, with head symptoms.

14. Ditto partial, marginal. Suppuration in sub-mucous tissue about epiglottis.

15. Ditto partial, slight marginal. Kidney granular and cysted. Death from injuries from a horse.

16. Ditto advanced state. Encephaloid disease of liver, testicle, lungs, &c.

17. Ditto advanced condition, combined with incipient cirrhosis. Phthisis; renal tubuli infarcted.

18. Ditto partial, slight marginal. Fatty degeneration of kidneys; cerebral affection, with dropsy after scarlatina; tuberculated bronchial glands; miliary tubercles in lungs.

19. Ditto partial, marginal. Maculated fever; kidney not healthy.

20. Ditto advanced stage, margins most affected. Psoas abscess; scrofulous caries of vertebrae; kidney to some extent degenerated.

21. Ditto very advanced stage, associated with partial cirrhosis. Tuberculization of lungs, not far advanced; fatty condition of kidney; urine albuminous. Died comatose.

22. Ditto partial, slight marginal. Ovarian tumour; hydrothorax.

23. Ditto complete. Circumscribed abscess in peritoneum; cretaceous tubercles in lungs; emaciation.

24. Ditto marginal, slight. Abscess in pelvis; peritonitis.

25. Ditto not quite complete in exterior two-thirds of lobules; cells gorged with oil, and breaking up. Bedsores and abscesses, sequelae of rheumatic fever; great emaciation.

26. Ditto far advanced, but cells not much broken up; those on margins most affected. Sores

		and secondary abscesses, after fever; pleural effusion; great emaciation.
27.	Fatty degeneration	partial, marginal. Acute tuberculization of lungs; vomicae; stupor aspect.
28.	Ditto	partial, marginal. Dropsy after scarlet fever; subarachnoid and ventricular effusion; recent pleuritis; anaemia marked.
29.	Ditto	partial, marginal. Extreme dilatation of bronchi.
30.	Ditto	partial, marginal. Ovarian tumour, multilocular and solid; emaciation.
31.	Ditto	general, advanced stage; margins most affected. Incipent granular kidney; dilatation of heart; dropsy.
32.	Ditto	general and advanced; margins most affected. Acute tuberculosis; pneumonia; prostration.
33.	Ditto	morbid oily *accumulation* in central half of lobules. Continued fever, 16 days; intestine ulcerated.
34.	Ditto	partial, marginal. Meningitis; pleuritis; abscesses in lungs and liver; injury to head.
35.	Ditto	partial, marginal, advanced, associated with cirrhosis. Heart enlarged, dilated, flabby; general dropsy.
36.	Ditto	advanced, but not quite general; associated with cirrhosis. Albuminuria; toxaemia.
37.	Ditto	partial, marginal. Recent acute pericarditis; diseased kidney; enlarged heart; pleurisy; pneumonia.
38.	Ditto	very advanced, without breaking up of cells; associated with some cirrhosis. Cerebral affection; meningitis, from inflammation of bones of cranium.
39.	Ditto	central, oily accumulation. Diseased kidneys; bronchitis; hypertrophied heart; pulmonary apoplexy.
40.	Ditto	marginal, or more general. Died with cerebral symptoms; lungs full of miliary tubercles.
41.	Ditto	marginal, very advanced. Died with some congestion of pia mater and haemorrhagic effusion at base of brain; kidneys granular and cysted; heart enlarged; dropsy.
42.	Ditto	marginal, with some cirrhosis. Incipent

		granular kidney; diseased valves of heart; blood fluid; dropsy.
43.	Fatty degeneration	marginal; a broad zone of oily accumulation. Phthisis; large vomicae; extreme anaemia.
44.	Ditto	complete, yellow stained. Died with fracture of pelvis, shock to system proving fatal.
45.	Ditto	complete. Long standing peritonitis; pneumonia; fatty heart; long suffered from debility and diarrhoea.
46.	Ditto	complete. Phthisis; kidneys healthy.
47.	Ditto	complete. Phthisis; kidneys healthy.
48.	Ditto	complete, universal; associated with slight cirrhosis. Kidneys not healthy; death from unhealthy suppuration after removal of scirrhous tumour from breast. The abscess formed in left thigh.
49.	Ditto	complete. Paralysis from non-inflammatory softening of spinal chord; no other disease.
50.	Ditto	marginal. Tuberculosis of lungs and other organs in a boy.

The conclusions which may be deduced from the foregoing Table are the following;—

1. That partial marginal fatty transformation of the lobules of the liver is peculiar to no pathological state, and can hardly be considered morbid.
2. That out of 20 cases of very advanced or complete fatty degeneration, 15 had no connection with pulmonary phthisis.
3. That in about 8 of the 20 advanced cases there was, in all probability, great emaciation; but that in the majority this had not taken place to any great extent.
4. That out of 20 cases of completely fatty liver, only 3 were associated with granular kidney; and in one only was there the coincidence with fatty kidney.
5. That of 30 cases of partial fatty transformation, there were 7 associated with granular kidney; but this seems only an accidental coincidence.

Postscript, August 14th, 1852.—In one or two instances since the above was written I have not found the administration of calomel produce any marked quantity of yellow matter in the liver cells. This cannot invalidate the positive results of the above experiments, but only shews that the action of the drug is not always identical, which is not more than might have been expected when various disturbing causes are taken into consideration.

Examination of Different Opinions as to the Value of the Climate of Madeira in Chest Disease. By GEORGE LUND, M.D., of Funchal, Madeira. [Journal of the Provincial Medical Association, 2d September 1853, No. xxxv., p. 767.]

[In volume seventy-fourth of this Journal, is given an account of the work of the late Dr Mason, on Madeira as a climate for the Consumptive.[1] The conclusions at which Dr Mason arrived, were rather adverse to the idea that Madeira is a beneficial and curative climate; and we thought it proper to adduce such evidence as the writings of other observers furnished, in order to enable readers to form for themselves a proper judgment on the question. Dr Lund, who has resided several years in Madeira, takes rather a different and a more favourable view. We have no other desire but that the truth should be known, and that the merits of Madeira as a residence for phthisical invalids should be properly understood. We subjoin, therefore, as much of the paper by Dr Lund as contains all the facts and arguments not previously known; and after this explanation, we leave the subject to the consideration of those interested, whether they are physicians or patients.]

Remedial agents used in the practice of medicine have, in all ages, had various degrees of importance attributed to them. In some cases, the assumption of the almost infallibility of newly introduced medicines in effecting cures has gradually led to their disuse; for, the exaggerated promises of the recommender not being fulfilled, the opposite view has become general. Thus, unmerited neglect has often been thrown upon many really useful adjuncts of the remedial art, which, highly beneficial when applied in proper cases and under right circumstances, have failed when used improperly: the error being unjustly attached to the remedy, and not, as it ought to have been, to its misapplication. The generality of mankind are too apt to look to specifics for the cure of ailments, overlooking the fact that diseased action rarely continues long the same. The products of diseased action, as effusions, depraved secretions, &c., often become themselves the disease to be treated, the primary state having ceased or changed. We thus ascertain that no one remedy can be universally applicable to the varying phases of a disease; and although it may be, and often is, an essential link in the chain of treatment, it can be correctly viewed only as contributing its subordinate though necessary help in assisting the whole means used, and aiding the natural powers to throw off diseased action, and thus insure a return to healthy function.

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[1] Edinburgh Medical and Surgical Journal, volume seventy-fourth, July 1850, No. 184, pp. 148–174.

Climate appears at the present time to be placed much in the position of the remedial agents just alluded to; at one time being too highly estimated, and by many looked upon almost in the light of "a cure," which necessarily has led to disappointment. Such an extravagant expectation not being realized, the idea has gradually spread that climate has little or no effect as a remedial agent, and its undue depreciation has thus ensued. Hence, in this, as in many other cases where opinions differ, we shall find that a middle course is nearest akin to truth; and that by following it we shall not be so far misled, as others who adopt extreme views. No one *personally* acquainted with the effects of a well selected climate in disease can deny its great efficacy in many complaints, especially in various chest diseases, and in consumptive cases, when these are sent out in an early stage, and the patient behaves with common prudence. Unfortunately, such favourable results induce a host of most unsuited, often literally dying people to try this remedy, of course for the most part unsuccessfully; and then climate is declared by the surviving friends to be a useless agent. To such an extent had this influx of unsuitable cases arrived at one time in Madeira, that the late Dr Renton published a paper, pointing out the great evils of the practice, and forcibly shewing the inutility and cruelty of sending far advanced cases abroad, in the vain hope of recovery,—a remonstrance which, for some time, had the beneficial effect of preventing such indiscriminate abuse of climate.

The beneficial effects of Madeira in suitable cases are well known to the resident medical men; but, to insure favourable results, the invalid must shew prudent conduct and follow hygienic rules. Such persons generally do well; and there is no doubt that many, who would have been in their graves if they had remained in a variable climate, have been restored to their friends by a temporary residence in one more genial, where, while avoiding most of the exciting causes of lung disease, they could invigorate the general health by means not available in England during the winter and spring months; thus enabling the system to throw the tubercular materials out of the blood, if they exist only there, and, if deposit has already ensued, to keep it unirritated and unsoftened, and so to arrest the onward progress of disease. I have witnessed, in cases very recently landed, and where the physical signs have been exceedingly slight, rapid and extensive tubercular deposit ensue in the lungs from excesses combined with imprudent exposure to the midnight air. Slight catarrh has come on; and this fixed irritation has appeared to be all that was necessary to cause a rapid accumulation of the deposit in the lungs. Had there been no imprudence, and no long irritation, in all probability the consumptive materials might, in the course of time, have been safely eliminated from the system, without being deposited in the lungs.

It is greatly to be regretted that much of the benefit of climate is entirely lost, from the imprudent conduct of many who come out, and act as if climate were to do everything, themselves nothing. These persons, content at home, and, whilst under the watchful eye of anxious relatives, considered and acting as invalids, on becoming their own masters abroad, throw off all previous restraints, freely indulge in various imprudences, and, when their disease has gained ground, declare (in Madeira, at all events) that their impaired health arises entirely from "the relaxing effects of the climate." Of these alleged effects, their friends and doctor at home are duly informed; nothing, however, being said of late dinners, evening parties, dances, concerts, cigars, and stimulants of various sorts, &c.; these, of course, not having, in their opinion, aided in breaking up the general health.

The leading members of the medical profession give sufficient evidence of their favourable opinion of climate, by recommending their patients to avail themselves of its benefits. In confirmation I merely refer to the opinions of Sir J. Clark and Dr C. J. B. Williams, and shall afterwards investigate the grounds upon which an unfavourable opinion has been advanced by others.

The late Dr Andrew Combe, so well known by his publications on the subject of preserving health, was most earnest in always inculcating and practically demonstrating the importance of hygienic rules for the preservation and restoration of health. Climate was always ranked by him among remedial agents of high efficacy, and he carefully secured its advantages in his own case; for, in writing to a friend, after having resided in Madeira, he says, "If I must go abroad, I shall most likely return to Madeira, simply on the ground that, if I must forego the pleasures of home, it is better to resort at once to the *most advantageous* climate, than to adopt the half measure of going to Italy, Jersey, or the south of England." (White's *Madeira.*) When in Italy, Dr Combe considered the climate as unsuited for invalids in an advanced stage of disease, though he did not take such an extreme view as Dr Burgess, a recent author on the climate of Italy, who declares change of climate in disease to be a delusion. He (Dr Burgess) writes, "There is no greater popular delusion than the belief in the existence of some undefinable specific virtue in the climate of Italy for pulmonary consumption. This 'foreign climate delusion' is not confined to the rich invalid;" he adds, "it is only when the disease is confirmed, and softening exists, that, in the great majority of instances, the patient seeks in a foreign clime that relief or cure, which he believes nature has denied him in his own."

Sir James Clark has explained that climate is not beneficial in such unsuitable cases; and that to use it only as a last resource is merely to abuse a remedial agent of acknowledged efficacy. When it is properly used, and under the most favourable circumstances, the invalid is to remember that the change only places him in a better position for the hygienic treatment of his disease; and that, without prudence and self-control on his part, little good will ensue from mere change of locality.

The idea of climate being a cure for phthisis, Dr Burgess says, is "a popular delusion." It is to be hoped that no medical man of the present day ranks it higher than a curative agent of great efficacy when properly applied. He remarks, in continuation, that the influence of climate on health and disease has of late attracted considerable attention; hence, instead of vague assertions or traditionary fame, authenticated facts are essentially requisite to establish the sanitary character of any given climate. At the end of his preface, Dr Burgess concludes thus: "Although several of the subsequent statements may be opposed to various popular notions prevalent in England, no opinion has been hazarded without the support of positive data; consequently, readers can judge whether the conclusions I have enunciated are fully warranted by the evidence upon which they are virtually founded." Fully admitting the importance of these remarks, and the great desirableness of receiving only well authenticated facts, I shall proceed, in compliance with Dr Burgess's request, to ascertain whether or not his conclusions with respect to the climate of Madeira, are erected on such a foundation as to warrant his readers in receiving them as correct.

Amongst other remarks the following: "That the popular faith in the virtues of this climate in pulmonary consumption is founded, for the most part, on tradition and romance; that, in short, the climate of Madeira, as regards the cure of phthisis, is as great a delusion as the climate of Italy." This is strongly put, and in forcible language; but I hope to give sufficient proof that, while the Madeira climate does not fulfil the popular delusion of being a cure, it is an agent of great curative efficacy in aiding proper treatment.

Dr Burgess, having no personal knowledge of the island, of its climate, or of the results of cases sent out, forms his opinions from two works, viz., White's *Madeira*, and Dr Mason's *Treatise on the Climate and Meteorology of Madeira;* and from the latter, the opinions of the late Drs Heinekin and Gourlay are incidentally brought in. Mr Harcourt's book on Madeira, published by Mr Murray at the time when Mr White's came out, is not mentioned; his opinion is in favour of the climate. Dr Burgess first quotes from White's *Madeira;* but, before proceeding to examine the extracts he has made, I may remark, that Mr White is described as enthusiastic in his praises of Madeira, and grateful for restored health. Any one having the pleasure of being acquainted with Mr White must be well aware that he is not of an enthusiastic disposition, and that he is peculiarly well qualified to afford correct information concerning Madeira, for the following reasons, viz., a residence of more than fifteen years on the island, his acquaintance with the people and their customs, and correct knowledge of their language. It is pleasing to find Dr Burgess recording Mr White's gratitude for restored health, attributed entirely to his sojourn in Madeira; and it is equally gratifying that I am enabled to add, that he is now living in continued good health in his native land,—one out of many proofs of the good effects of climate.

In the quotations which Dr Burgess makes from Mr White's book, he writes, that Mr White is obliged *reluctantly* to make the following confessions:

1st, That pulmonary consumption and scrofula do occur, but *less frequently* than in more changeable climates.

2d, That there are different eddies or currents caused by the vicinity of mountains, which render a vane or anemometer of little use.

3d, That the sky is not so clear, nor the atmosphere so calm, as in Italy.

I shall consider these remarks in the order in which they are placed above.

First, with regard to the statement, "that consumption and scrofula do occur (in Madeira), but *less frequently* than in more changeable climates." To proceed with Dr Burgess's extract: "Mr White, a *non*-medical writer on the subject, is enthusiastic in his praises of the climate, and evidently grateful for the blessing of restored health, which he attributes entirely to a sojourn at Madeira." He (Mr White) "remarks, without advancing any pretensions to medical knowledge, or the physiological effects of climate, that a residence of many years at Madeira, and a lengthened pursuit of health among the most favoured localities of Europe, enable the writer to add his testimony to the decided superiority of the climate of Madeira over all those he has visited. Cold winds, or close, sultry weather, are little known; and a continuous summer may be enjoyed, without suffering from extreme heat or cold, or a continuance of damp or wet weather. The most remarkable feature of the island is probably the mildness and equability of its climate, and its consequently beneficial effects in pulmonary and other complaints."

Dr Burgess remarks upon the preceding;—"Notwithstanding this eulogium, the writer (Mr White) has some misgivings as to the perfection of the climate, and reluctantly admits that, though so 'very equable,' the climate of Madeira is not altogether free from changes, which constitute there, as well as elsewhere, the exciting causes of pulmonary affections. Pulmonary consumption and scrofula occur among the natives of Madeira, but *less frequently* than among the natives of more changeable climates."

Upon referring to Mr White's book, chapter ix., it appears that Mr White is not the author both of the eulogium and subsequent misgivings as to the equability of the climate; for it is expressly mentioned, that the following brief remarks were obtained from one of his medical friends, who gives his opinion thus: "Although the climate of Madeira is so very equable, it is not altogether free from changes which constitute here, as well as elsewhere, the exciting causes of pulmonary and inflammatory affections." Dr Burgess omits the following, which follows in continuation: "These, however, as may be supposed, are comparatively rare among the better classes, and occur chiefly among the hard-working poor, who are more exposed to this cause, and to sudden chills of the surface while perspiring profusely. An epidemic catarrh not unfrequently makes its appearance about the months of August and September, or before the annual rains which usually occur in October. It is now pretty well understood, that no climate affords entire exemption from scrofulous disease. Whenever the exciting causes are applied, in whatever part of the world, these diseases manifest themselves. Pulmonary consumption is connected with what is called the scrofulous diathesis, and occurs among the natives of Madeira, but prevails much less among them than it does among the natives of more changeable climates." That it should occur is not to be wondered at, when we find many of the causes of the scrofulous diathesis in full operation among a large proportion of them, viz., imperfect food and clothing, deficient ventilation of their low, damp huts, combined with overwork.

A short extract, like the one made by Dr Burgess, seldom affords a clear insight into an author's meaning; and I trust that, by supplying the sentences entire and unmutilated, the writer's real meanings have been placed in a more distinct point of view; and that, being so viewed connectedly, they simply shew the well-known and acknowledged fact, that *causes* of diseases, such as imperfect food, scanty clothing, deficient ventilation, damp wet dwellings, joined to overwork, do exercise their baneful influence in Madeira, as elsewhere, but with this important limitation, that they do so with less effect than in other places; that scrofula and consumption occur among the natives of Madeira, but *less frequently* than in more changeable climates. The diseases attributed to the changes in the weather are especially noted in Mr White's book to be "pulmonary and inflammatory, chiefly occurring amongst the poor hard-working people, from sudden chills, causing checked perspiration."

The second quotation extracted by Dr Burgess is also imperfect, from beginning in the middle of a sentence. It is thus given: "Yet, in this almost perfect climate, the same writer (Mr White) informs us that the different eddies or currents render either a vane or anemometer of little use." The *entire* sentence gives the reason: "From the position of the basin of Funchal, open only towards the south, it is difficult to ascertain the true course of the winds, and [here Dr Burgess only begins] the difficult eddies or currents caused by the vicinity of mountains render either a vane or anemometer of little use."

Dr Burgess's third quotation is to the following effect: "That the sky cannot be generally so clear, nor the atmosphere so calm, as that of Italy, from the position of the island, and nature of its surface." I again supply the whole sentence, as the extract begins in the middle of one, after a comma: "From the position of the island of Madeira, and the lofty and rugged nature of its surface, it will appear sufficiently obvious to every one conversant with the causes of the formation of clouds and the trade wind, [here Dr Burgess commences the extract] that the sky cannot be so clear, nor the atmosphere so calm as that of Italy." Mr White proceeds: "The preceding tables, however, though compiled during a season of unusual severity, certainly one of the coldest for a great number of years, still afford a favourable result."

We subjoin Mr M'Ewen's table of the comparative appearance of the sky, from Dr Mason's work, and quoted by Mr White.

Blue or clear sky, . .	240	times
Cloudy, ...	223	„
Hazy, ...	30	„
Overcast, . . .	30	„
Threatening, . . .	19	„
Rain, ...	13	„
Showers, . . .	11	„
Fogs (none) . . .	0	„
	566	observations.

Thus, showers or rains fell twenty-four times in 566 observations. The clouds rarely descend lower than the Mount Church, an elevation of more than 1800 feet; and form a most necessary and agreeable screen from the sun's rays.

The atmosphere is stated by Dr Burgess not to be so calm. I append the following table in refutation.

Calms, . . .	102
Light winds, . .	130
Fresh winds, . . .	82
Strong winds, . . .	9
Presumed gale, . .	1
	324

So that, out of three hundred and twenty-four observations in this climate, alleged to be deficient in calmness, we find only nine strong winds, and one presumed gale.

Dr Burgess gives, on the authority of Dr Mason, the following quotation, with reference to the variability of the weather at Madeira, and adds, that it will perhaps surprise his readers. If the reader has ever resided at Madeira, we think he will be astonished at the following statement. "The very frequent and remarkable variations in a series of years, incontestably prove that Madeira is no more to be relied on than any other place for certainty of fine weather; and that it has equally its annual variations of temperature." Dr Burgess's own opinion as to the requisites in climate for an invalid is as follows: "It is not heat, but equality of temperature, even if it were at a low range, that is needed by the class of invalids referred to; and, whenever an approximation to that quality of climate can be found, there should the consumptive invalid reside." I quite agree with Dr Burgess's opinion, that equability is an essential element in any climate used for invalids with delicate chests; and I think that Madeira supplies that desideratum, in addition to having home comforts which most other foreign places of resort sadly lack. Certain I am, that changes injurious even to delicate persons do not occur there, and that the most sensitive invalid need never suffer more than a temporary feeling of discomfort. True it is, that invalids often ascribe their various ailments to the effects of climate, which would be more justly ascribed to their own frail bodies, and to the usual complications of disease.

As to the variability of the Madeira climate, alleged by Dr Mason to exist, we extract the following, to shew that other observers do not (as in other observations) confirm his opinion. Sir James Clark, on the authorities of Drs Renton and Heineken, both scientific men, and many years resident on the island, writes thus: "When we take into consideration the mildness of the winter, the coolness of the summer, together with the *remarkable equality* of the temperature during the day and night, as well as throughout the year, we may safely conclude that the climate of Madeira is the finest in the northern hemisphere." The winter at Madeira is 20° warmer than at London, and has no fogs; the summer is only 7° warmer, and is free from the oppressive closeness so common there. "No one," writes Sir James, "need breathe, night or day during winter, a lower temperature within doors than 64°, or in the country 74°." That is only ten degrees of mean difference of Fahrenheit between summer and winter; whereas a summer day in England has often greater changes. The following tables shew how little variability of temperature there is; two of the year's observations having been made nearly one hundred years ago.

Date.			1847.	1749.	1750.	1850.
January . . .	59·50	57·71	63·50	—	64·00	61·00
February . . .	58·50	60·28	63·14	—	63·80	61·14
March	61·06	61·86	64·22	64·66	66·50	62·63
April	62·50	63·03	64·40	66·07	66·47	66·50
May	63·50	63·44	64·76	66·53	66·25	67·33
June	65·00	66·90	68·72	68·75	69·06	66·19
July	70·00	70·04	72·50	74·58	73·00	69·01
August	73·00	71·88	73·58	75·07	75·40	69·79
September . . .	71·50	71·28	71·94	76·53	74·93	69·24
October	67·50	66·76	71·06	72·20	73·87	69·58
November . . .	62·70	63·96	67·64	68·60	70·82	66·00
December . . .	60·50	61·44	63·86	64·90	66·27	62·51
Authorities:	Sir J. C.	White, Harcourt.			Mr White. (Interval of a century.)	

Sir James Clark states the mean annual temperature of Madeira to be 64·56° Fahr.; the mean difference between summer and winter, 9° 38′; mean difference between the coldest and hottest months, 14° 50′; the mean difference between successive months, only 2° 41′; difference between winter and spring, 2° 70′; between spring and summer, 7° 13′; between summer and autumn, 2° 10′; between autumn and winter, 7° 73′. The differences of successive months are: between January and February, 1°; between February and March, 2° 56′; between March and April, 1° 44′; between April and May, 0° 50′; between May and June, 2°; between June and July, 5°; between July and August, 3°; between August and September, 1° 50′; between September and October, 4°; between October and November, 4·80°; between November and December, 2·20°; between December and January, 1°.

Mr Harcourt, writing from his personal experience of the climate, gives his opinion as follows. "The mean temperature of Funchal throughout the year may be stated at 66° Fahr.; February and March being the coldest, August and September the hottest months. Even between these months there is not a greater mean difference of temperature than 12°. It is this uniformity in which the excellence of the climate consists. The causes which are instrumental in forming such a climate in Funchal are threefold: 1st, The lofty hills, which immediately surround it on the north, completely shelter it from the weather at all points of the compass except from south-east to south-west. 2dly, The absence of wood, while it impairs its beauty, improves its climate; and, 3dly, the regularity of the alternations of the land and sea breezes tends to preserve a delicious equability of temperature."

"This climate is certainly fine enough to please even fastidious people. A range of 12-14 degrees between summer and winter, surely does not amount to 'frequent and remarkable variations;' and even this small amount of difference between the hot and cold months may be greatly, and almost entirely, avoided, by going out of Funchal during the summer, which it is the custom to do. In the mountains the summer heat is less than in England; but, at this elevation, there is too much damp for an invalid, though healthy persons enjoy such mountain air, and are braced by it. Lower down the air is drier, and an invalid can have summer with the thermometer rarely exceeding 72° or 74°." In the Isle of Wight, the most favoured of English places for consumptive persons, the difference between the hot and cold months is 28 degrees, being fully double what it is at Funchal.

I shall now proceed to investigate the extracts in Dr Burgess's book, taken from Dr Mason. Before, however, detailing the results of Dr Mason's observations, Dr Burgess informs us, that the instruments used were improperly placed, a fact which alone vitiates the whole of the results. The instrument chiefly used was the hygrometer, which is very easily affected by the slightest causes, arising from local currents of air, moisture, or ill-chosen locality: and it is much to be regretted that so much zeal and industry as the late Dr Mason exhibited, should be rendered of no avail, not from one, but from many causes of inaccuracy. Dr Burgess says: "I shall now place before the reader the leading facts contained in Dr Mason's book;" and a little farther on, "a summary of Dr Mason's observations will enable the reader to form his own opinion as to their merits, and the *reliance* to be placed on them." Mr White, as usual, explains matters more fully and correctly. He writes: "The situation, where Dr Mason's observations were made, is notoriously one of the least favourable for that purpose in the outskirts of Funchal; and this circumstance, together with the fact of his hygrometer being placed in a close room, between two windows, near an open tank, and surrounded with trellised vines and vegetation, independent of the acknowledged severity of the season during which they were made, will sufficiently account for the difference existing between his observations and those of other parties." Independently of other sources of error, the immense exhalation from the leaves of plants would vitiate hygrometrical observations. An experimenter, placing his instruments in a place which has marked peculiarities, can obtain only results appertaining to that locality, and cannot correctly affirm, that observations thus made truly indicate the *general* climate of a country. No one would place a hygrometer in a Lincolnshire fen, or an Irish bog, to decide the general humidity of the climate in either country; nor can more weight be justly allowed to Dr Mason's observations, than that they indicate the humidity of the atmosphere near his own water tank. At one time it was thought that the greater humidity observed by Dr Mason must have originated in the observations having been made in a damper season than usual; but this opinion is apparently erroneous, because the year in which they were noted, 1834–35, was a particularly good wine year, and, to secure a good vintage of full flavour, a dry season is necessary; consequently we must look to other sources for the discrepancies observed between his remarks and those of other persons. On the island, it is well known that the locality is damper than the neighbourhood around it; but the accuracy of the whole series of observations is vitiated chiefly from the improper position of the instruments, &c. Dr Mellermair, who has made careful observations, informs us, that the results obtained by him confirm Mr White's, and that both prove Dr Mason to have placed the humidity of Madeira one-third too high.

The following letter, from Mr Wilkinson of Madeira, more fully explains the above.

"*Funchal*, 1*st June* 1853.

"SIR,—In reading the book on Madeira, by Dr Mason and Mr Driver, edited by Sheridan Knowles, the suspicion occurred to me, that the conclusions there made with regard to the climate of the island, and drawn from the meteorological tables said to have been kept by the late Dr Mason, were not trustworthy.

"I was personally acquainted with the late Dr Mason, and had the pleasure of enjoying a good deal of his society while he resided in Madeira; and I know he did not reside at Sta Luzia during the whole time the tables are given as having been kept there by him, but that he made frequent and long excursions into the country. Neither, to the best of my recollection, did he leave any one to carry on the tables during his absence. True, he always carried a small thermometer and hygrometer with him into the country; but that could have nothing to do with observations on the climate of Funchal or its neighbourhood.

"When Dr Mason commenced his observations on the temperature, &c., of the atmosphere here, it was solely, I believe, to assist him in some improvements he was attempting in hygrometers; but, at the same time, he noted down his conclusions with regard to the value of the climate in the treatment of disease. Certainly his means of arriving at these conclusions were very imperfect. His attention was chiefly directed to the working of a Leslie's hygrometer, and of a common thermometer, hung in a cupboard. All this, of course, casts no imputation on Dr Mason; as Mr Driver and the editor of the book are alone answerable for the use they have made of the imperfect notes left by that gentleman at his death.

"Another circumstance that would make his observations of little value for hygrometrical purposes, was the situation in which his instruments were placed; viz., a corridor about eight feet from the ground, and about fifteen feet from a tank holding about three hundred gallons of water. The water in the tank was renewed about every fifteen days, by a stream running into it; but was never entirely emptied, as it contained fish. The tank is of stone, entirely open at the top, and nearly on a line with the corridor. The surface of the water lies exposed to the afternoon sun, and to the current of air from the valley of Sta Luzia. In fact, no one, who had a choice, would choose such a situation for purely meteorological purposes.

"All this is matter of personal knowledge with me; and you will easily understand why I should be of opinion that the late Dr Mason, had he lived, would never have published his notes in the form we find them in this work. I remain, Sir, yours truly, SAMUEL WILKINSON.

"*G. Lund, Esq., M.D.*"

The objections made to the climate of Madeira, on the authority of Dr Mason, may be classed under the following heads: dampness, rains, injurious effects of the l'este, and precipitation of dew.

It is mentioned that London, at its mean temperature of 58°, can contain 100 parts of moisture; whilst Funchal, at 68°, can hold 200. Both calculations being made by Fahrenheit's thermometer, commence at the freezing point, 32°; add 18°, and we have the London temperature; double the temperature by 18° again, and we get 68°, the Funchal temperature, simply shewing that double the heat and double the quantity of moisture will be suspended in the atmosphere.

Further, in Madeira it is suspended, and is never visible in the shape of fogs, except on the mountain tops.

Mr Harcourt thus expresses his opinion on the alleged damp of Madeira: "Whatever may be the moisture of the air, it is not sensible as dampness to the feelings, and certainly is not unfavourable to the healthy action of the lungs. There may be spots in Madeira, either near yam grounds, where much irrigation is carried on, or where the soil and rock, usually very porous, are so close as to retain the wet, which may be too humid for some constitutions; but, for the greater part, in the neighbourhood of Funchal, there is no visible excess of moisture, there is no fog, and there is comparatively *little dew*. If dampness has ever been alleged as an objection to the salubrity of this island (Madeira), it must be regarded as a complaint due to the unlucky choice of some dwelling-house unfavourably placed, or to the common fastidiousness of ill health."

Dr Mason pronounces Dr Heineken in error, for stating "that at the level of the city of Funchal, no perceptible dew is produced, but up the mountains it is profuse."

It is surprising how Dr Mason could have committed such a mistake; for every resident is well aware that Dr Heineken is in the right, and Dr Mason glaringly in the wrong. Sir J. Clark gives the following direction: "Invalids particularly sensitive to humidity, are recommended to live within the limits of the town, on account of its greater dryness." Again, Dr Mason makes the following erroneous statement: "That, on a clear night, the quantity of dew precipitated is so great, that in a few hours many drachms of fluid may be collected." This is palpably incorrect, the experiment having been often tried in other localities, without the plate being even damped.

The l'este, a warm wind from Africa, which is not common, is described by Dr Mason as injurious in its effects. He says that at one time it made him very well, and at another very ill. We presume that this opinion is of no more value than as it denotes the morbid feelings of a nervous invalid. The l'este does not occur every season, and is felt to produce only temporary inconvenience, and that in summer, when few invalids remain on the island: its general continuance varies between three and six days. Never having had to prescribe for any one made ill by the l'este, I conclude that Dr Mason has overrated its bad effects. Dr Mason corroborates the opinion of Dr Gourlay, as to the frequency of phthisis among the Madeirans: the relative value of these may be gathered from the following.

Dr Mason did not speak the language of the people whose diseases he affirms that he knew, nor did he practise amongst them ; consequently his opinion must have been secondhand. Dr Gourlay did practise amongst the Portuguese, but *before* the stethoscope was used on the island; consequently his means of detecting chest disease must have been very imperfect. The late Dr Renton, an excellent physician and good stethoscopist, writes : " With respect to the question relative to the frequency of consumption among the natives, Dr Gourlay, if he alluded to tubercular disease, has greatly overrated it." Dr Heineken also says : " It has been asserted that no malady is more prevalent than phthisis with the natives of Madeira; but, so far as my own personal experience and the result of inquiries go, I incline to a contrary opinion." (Clark.)

Dr Burgess thus sums up : " Madeira, with all its sanitary fame, is no exception to this rule,"—that is, it affords no proof of salubrity, or of the beneficial effects of climate, —" as (to pursue the quotation) the meteorological observations of Drs Heineken, Gourlay, and Mason, incontestably prove." Setting all other considerations aside, we do not think that these three observers bear out this assertion in their own cases. Dr Gourlay was not consumptive. Dr Heineken was originally sent out as a dying case, lived nine years on the island, and ultimately died from accidental exposure to the night air in an open boat, when returning from an adjacent island. Dr Renton opened the body, and was astonished to find how little lung he had left. Dr Mason, notwithstanding his neglect of means for preserving or obtaining health, and devoting himself instead " most assiduously to meteorological observations," lived in Madeira two years, this time being the full average duration of the whole different stages of phthisis in England. After this, he went to Nice. We are informed that he first visited Madeira with the belief, that he would recover his health, under the alleged sanitary influence of that climate. Whilst there, he most assiduously occupied himself in meteorological investigations. From this, the conclusion at which Dr Burgess arrives is, " that the climate of Madeira, as regards the *cure* of phthisis, is as great a delusion as the climate of Italy." Previously, we were informed that the idea of *cure* was a popular delusion ; but this shews its adoption by at least one medical man. The belief of medical men generally is, that no climate can cure ; but that it is an essential and important aid of other means of treatment, which cannot be so efficiently carried on in a variable climate, especially during the winter and spring months.

Dr Mason, it is stated, always felt persuaded that Nice was a place better adapted to his case than Madeira. Why he remained at Madeira two years, the ordinary duration of a consumptive case in England, with this impression on his mind, and of course, in his opinion, losing vital time, is not explained. Dr Burgess continues: "The fate of the author was a melancholy one, and a telling comment on the blind credulity which prevails respecting the virtues of foreign climates in pulmonary consumption." During his journey to Nice, he travelled twenty-four consecutive hours in a diligence, his only sustenance, during that period, being fruit und bread, *accidentally* obtained on the road; upon arriving, dysentery (it is said) attacked him, and proved fatal in a fortnight. Dr Burgess remarks on this catalogue of imprudences committed by a dying invalid: "How many consumptive invalids have fallen victims abroad to the same delusion." In my opinion, the delusion would have lain in expecting any other result than what happened. Over-fatigue, with improper food, in an advanced stage of consumption, produced the usual diarrhœa, (not dysentery), and speedily proved fatal; nor, in fact, could any other termination be looked for.

We have now to consider how far the assertion is correct, "that the popular faith in the virtues of the Madeira climate is founded for the most part on tradition and romance." The following tables, I think, prove the contrary, and shew that climate, in the majority of cases, saves the life of consumptive people, when they are sent out in the early stage, and before the disease has worked irreparable mischief. The tables of confirmed phthisis speak for themselves.

Dr Renton's Tables, extracted from Sir J. Clark's work on Climate.[1]

TABLE I. CASES OF CONFIRMED PHTHISIS, 47.

Died within six months after landing in Madeira,	32
Went home in summer, returned and died,	6
Left the island, of whose death we have heard,	6
Not since heard of; probably dead,	3
	47

TABLE II. WITH TUBERCULAR LUNGS, 56.

Died here,	30
Left the island,	22
Still here,	4
	56

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[1] The first and third of these Tables were originally given in 1827, in the twenty-seventh volume of this Journal, p. 305.

TABLE III. INCIPIENT PHTHISIS, 35.

Left the island much improved, . .	26
Improved, but not since heard of, . .	5
Have since died, ...	4
	35

TABLE IV. THREATENED WITH PULMONARY DISEASE, 108.

Remained free from symptoms, . .	93
Fell off, ...	13
Lost sight of, ...	2
	108

Table by Dr Lund, from White's Madeira, *shewing the number of patients, in* 100 *cases of Consumption, landed in different stages of the disease, in whom it was arrested, progressed, aud proved fatal.*

FIRST STAGE: 48 CASES.

Arrested—37.

Duration of arrestment.	From 4 to 10 years, . .	13	
	For 3 years, . . .	2	
	From 8 to 20 months, . .	11	
	From 7 to 12 months, . .	11	
		—	37

Relapses occurred in 2 cases.

Progressed—11.

Living.	Passed in 14 months into third stage, and slowly progressing, . .	3		
	Passed in 16 months, 2 years, and 5 years respectively, into second stage, .	3		
		—	6	
Died.	Died 5½ months after landing, .	1		
	Remained one winter at Madeira, and died the next winter abroad, .	1		
	Went out for 7 winters, and then died,	1		
	Remained nearly 8 winters, and then died,	1		
	Remained 1 winter, returned home, and supposed dead, . .	1		
		—	5	
			—	11
				48

SECOND STAGE: 24 CASES.

Arrested—5.					
	Duration of arrestment.	Arrested for 15 months; relapsed, and passed into third stage. Disease again stopped for 3 months; general health as good as before last attack,	1		
		For 15 months,	2		
		For 5 years,	1		
		Came out about 10 years ago; relapsed in 1847; disease again arrested, and continues so,	1		
			—		5
Progressed—19.					
	Living.	Much ameliorated,	2		
		Still remain in the second stage; but the disease is slowly gaining ground,	4		
		In the third stage; but the disease is arrested, and general health good,	2		
			—	8	
	Died.	Remained one winter; and, having left the island in a sinking state, supposed dead,	1		
		Remained one winter at Madeira, and died the next winter at home,	4		
		Died at home, 8 months after first landing in Madeira,	2		
		Ditto, 10 months, ditto,	1		
		Ditto, 12 months, ditto,	1		
		Died at Madeira, 14 months after first landing,	1		
		Ditto, 4 years, ditto,	1		
			—	11	
				—	19
					—
					24

THIRD STAGE: 28 CASES.

Arrested—5.					
	Duration of arrestment.	Remained so for upwards of 12 years,		1	
		For 8 years,		2	
		Left the island after 3 years,		2	
Progressed—23.				—	5
	Living.	Arrested for 14 months, and then began slowly to progress,	1		
		Has progressed slowly for 15 months,	1		
		Remained one winter, and then left, their only symptoms being moderate morning cough and expectoration,	3		
			—	5	

Died.	Died 48 hours after landing .	1		
	„ 6 weeks „ . .	1		
	„ 7 „ „ . .	1		
	„ 9 „ „ . .	1		
	„ 3 months „ . .	3		
	„ 3½ „ „ . .	1		
	„ 4 „ „ . .	2		
	„ 5 „ „ . .	1		
	„ 6 „ „ . .	1		
	„ 7 „ „ . .	2		
	„ 10 „ „ . .	1		
	Died 15 months after first landing in Madeira; went home in summer, and died 3 months after returning, .	1		
	Died 4 years after first landing, .	1		
	Came to Madeira 13 years ago, and remained 7 winters; went home for 3 years; returned to Madeira for 3 winters and 2 summers; and died at home, .	1		
		—	18	
			—	23
				—
				28

Living.	First stage,	43	66	= 100
	Second stage,	13		
	Third stage,	10		
Dead.	First stage,	5	34	
	Second stage,	11		
	Third stage,	18		

It is well known that, in chest disease, the general health must be invigorated as much as possible; and that injurious causes, exciting lung irritation, as cold, damp, and variable weather, are all especially hurtful, and carefully to be avoided. Now, in such a climate as that of Madeira, these injurious changes, and their consequent lung irritations, rarely, if ever occur; and, when lung irritation does come on, it is much milder than at home; and there is this further advantage, that the tone of the system can be improved at the same time, by means not available in Great Britain during the winter and spring months. Cold or tepid bathing, sponging baths, moderate clothing, constant out-of-door exercise, riding, boating, sitting in the open air, or with open windows, are habitually used.

From the preceding observations and tables, we may consider it fully proved, that the opinion long held and expressed by the more experienced members of the medical profession, regarding the beneficial effects of a genial climate in chest disease, is the correct one; and that the statements promulgated against that opinion are not supported by facts, certainly not so far as relates to Madeira, and most probably not to Rome, Pau, or Egypt, with the condition that properly selected,—not dying,—patients are sent to these localities. So far as regards Madeira, I could strengthen the details of its beneficial effects by relating individual cases; but that would unnecessarily lengthen this paper. Of one thing I am fully convinced, after many years' residence in Madeira, namely, that, in the early stages of phthisis, a person has in that place an infinitely better chance of having the disease arrested, than in England, or any other place that I am at present aware of; and that, in the latter stages, the disease is much delayed, the invalid enjoying all the comforts of life before finally sinking; and that a few cases occur, where the prolongation of life is very great. Many in Madeira live longer, say from three to four years, than the average duration of the whole three stages in Great Britain, which is limited to a period extending only to from eighteen to twenty-four months. I have cases where this retardation of disease has extended to ten, twelve, and even twenty years; and one to twenty-five years. And many have lived on the island in perfect health, whose brothers and sisters have, from a contrary cause, all died; or have originally come out as phthisical, and have lived free from all tubercular complaint.

ART. VIII.—*Report on an Attack of Epidemic Cholera in the Central Jail at Agra in* 1851. By JOHN MURRAY, M.D., Civil Surgeon, Agra.

The history of the progress of Cholera in India during the last ten years will assist in explaining the origin of the attack which forms the subject of the present report.

I have seen cases of Cholera every year since my arrival in India in 1833; and there are few, if any stations in India, where there are not some cases each year. As the symptoms are essentially the same, it will be necessary to state why I have separated this from the ordinary disease of the country. It appeared in certain localities for a few weeks during the hot or rainy seasons, and disappeared during the cold season. These were geographically contiguous, and were progressively affected. The disease advanced from north to south, branching off to the west, and it is now returning from south to north, branching off to the east.

The disease appeared very generally, and in great intensity. For example, there are usually 10 or 12 cases at different periods during the *year* in the Agra jail; this year there were upwards of 200 in a *fortnight*. During the hot season of the year 1842 Cholera was very general and fatal at Cabool. It was called the Black Plague. During the same season in the year 1843 it prevailed extensively in Peshawur; and in 1844 it extended to the Punjaub, and was very fatal in Lahore. It reached the British territories in 1845, and proved very fatal at the following stations successively: Ferozapore, Loodianah, Umballah, Soobathoo, and Meerutt. In September that year there were a few ordinary cases at Muttra, where I was stationed. During the following hot season it appeared at Gwalior, having apparently passed in the cold season, and left Agra intact. During this year it appeared to have descended the Sutledge, as it was very fatal at Kurratchee. In the year 1847 the disease was very fatal in Malwah, where I saw it in August. I am not acquainted with its progress during the two following years; but in 1850 it reappeared in Malwah, and returned in a northerly direction to Gwalior, where it was very general towards the end of the hot season. During the rainy season it extended to Dholepore and the banks of the Chumbul, where it depopulated several villages, and had extended to Munnea, 22 miles from Agra, where a heavy fall of rain took place in the end of September. This appeared to arrest the progress of the disease, as there were no fresh cases. It appeared again to pass Agra during the cold season, as it reappeared at Hattras and Allyghur during the last *hot* season, viz., May and June 1851, but it broke out in Agra in July. It also appeared at Bandah, Allahabad, and Benares, during the hot season, and at Lucknow very severely during the rains. During this year the disease appeared in great virulence at Bussorah and on the Euphrates. This may be a continuation of the current which passed to the westward, and reached Kurrachee in 1846, whose progress would be through Turkey to Europe. At this rate it would appear on the shores of the Mediterranean in 1852 or 1853.

At Agra the cold season 1850–51 was cloudy and wet, with little very cold weather. The hot season was steady; it was very hot and dry till the 1st July, when the rainy season set in. There were rain or heavy clouds uninterruptedly till the

9th of August, when it cleared up till the 31st; there were several heavy showers up to the 12th September, after which a steady dry westerly wind set in and continued during the month, and brought agreeably cold weather.

The prisoners were very healthy during the spring and hot season. Hospital Gangrene, which had been prevalent during the preceding rainy and cold seasons, had disappeared. During the month of July, previous to the 25th, there had only been 17 admissions from fever, and 13 from bowel complaints, 5 from other diseases, and 22 from boils and sores.

The first case of Cholera appeared on the 25th July, and the second on the 26th. There was no case on the 27th, but on the three following days there were 10 admissions. On the 31st there were 15 cases, and up to the 14th August that was the average number of daily admissions. The greatest number was on the 5th, when 19 were admitted. From the 15th to the 26th there were a few admissions daily, and on the 29th and 30th there were 5 more cases, and 2 in the beginning and 2 more towards the end of September, after which no fresh cases appeared amongst the prisoners or their guards. The disease broke out after 25 days of continued rainy and cloudy weather; whilst the rain continued, the disease raged, and after the weather cleared up, the disease rapidly disappeared. This differs from the usual course of Cholera. It generally appears in hot dry weather during the hot season, or in the hot clear intervals after the first fall of rain, and towards the end of the rainy season.

The first case admitted from the city of Agra was in one of the guard, on the 29th July. The disease was more prevalent in the villages on the outskirts of Agra than in the city, and it became most fatal after the rain ceased. I append a table (No. 6) of the mortality of the city of Agra for the month of August, as registered by the magistrate, shewing a considerable increase in the number of deaths. Much reliance cannot be placed on the names given by the police to the diseases that proved fatal; but it may safely be inferred that the increase in the mortality was from Cholera. The disease gradually subsided as the weather became cool towards the end of September. This mortality in 2 months of 908 in a population of 66,000 in the city and 40,000 in the suburbs is not great. The number of inhabitants is not accurately known; but there are 11,605 houses in the city, and 13,475 in the suburbs. The superior *cleanliness* and improved ventilation from opening out new wide streets, prevented it from spreading in the city so extensively as in the less clean suburbs. It was severe in the villages in the vicinity of the jail in the beginning of August, viz., Wezerepore to the north-east, and Saidkow to the south; and there were several cases after the 10th August in the Convent and Orphanage. In the Female Orphan School there were 12 cases, of which 2 were fatal; in the Nunnery, 1 case.

There were very few cases in the cantonments of Agra, and these occurred after the weather had cleared up. There were twenty-nine cases in the 2d European regiment, of which seven proved fatal. Amongst the native corps, there were only fifteen cases, and three of these were from the company of sepoys on duty at the jail. Amongst the out-lying gangs of prisoners, the first case from Secundra was admitted on the 1st August, and, on the same day, a case from Sahagunge. The first case from the Taj was on the 3d August. The most marked distinction is, that the four wards in the centre of the jail were least affected. The proportion of cases, and the mortality amongst the life prisoners and those of long terms is great, compared with those of short terms of imprisonment. Of the latter class, about 500 were at Secundra and the Taj, 500 with the magistrate, and 400 at Sahagung, Rambag, Nowmilah, and other outgangs; and amongst these there were only 54 cases and 16 deaths. The number of prisoners included in the first class was nearly similar, viz., about 1500; but, from the nature of their crimes, they were not allowed to leave the jail or its immediate vicinity. Amongst these there were 199 cases and 59 deaths.

The disease had evidently a connection with the locality; and under the impression that removal would prevent the men being affected, I sent 300 to Secundra and the Taj on the 31st July, and of these only two were affected. For similar reasons, and from observing the immunity of those sent away, I wished to send 300 more away on the 1st August from the jail into the fort at Agra; but the military authorities objected to their being admitted, lest they should contaminate the garrison. Had the disease continued after the rain ceased, I would have removed the prisoners into tents.

The disease was evidently in some degree dependent on the locality, and probably modified by the diet, cleanliness, and ventilation of the jail. The jail is situated in an open plain, with a deep tank to the north-west, well drained, and no marshy ground in the vicinity. The wards are well raised and freely ventilated. When the rains set in, on the 1st July, 2039 slept in the jail; of these, 200 were sent to Secundra and the Taj, as the barracks for their sleeping were not finished. On the 31st July and 1st August, when the

Cholera broke out, 300 more were sent to these outgangs, leaving 1556 remaining in jail. As this is one quarter less than the number that occupied the buildings in perfect health a few days before the Cholera appeared, crowding or want of space and ventilation cannot have caused the disease.

The diet consists of bread and Dhal vegetables and chubena, with spices and tobacco. During the cold season they received coarse flour in increased quantity; but since the 15th April they had got the finest wheat-flour. The diet was the same as the prisoners outside used, who suffered slightly, and it was totally different from what the European sergeant and our Christian prisoners used inside; and the former died, and the latter had an attack of the disease. It therefore cannot be attributed to the diet.

In regard to cleanliness, the excrement and filth are carried out morning and evening to pits half a mile distant, and there were no disagreeable smells perceptible in the jail, nor any variation from former years when there was no Cholera.

Though the disease did not originate from defect in any of these important points, I am satisfied it would have been more fatal and prevalent had either of these points been neglected. The disease increases in impure air; it is concentrated in confined air, and it readily attacks one weakened by insufficient or indigestible food; but these circumstances alone will not induce the disease, nor will their opposite always ward it off. What is the disease? I have given its history over a considerable portion of Asia of late years, which bears an analogy to that of its appearance in Europe twenty years since.

I shall now give its most prominent symptoms, though these are not constant. I have seen a strong man, after a single watery motion, in a few minutes become livid, cold, and pulseless; he still breathed mechanically for a few hours, as the pendulum of a clock will vibrate for a few minutes after the mainspring is broken. If roused, the mind was still clear, but its light was gradually eclipsed till darkness followed, and night remained. In some instances, the symptoms disappeared as rapidly as they had supervened, leaving no train behind. These are the extreme cases. The ordinary symptoms were watery vomiting and purging, with a sinking pain in the region of the heart and stomach, with cramps of the extremities and body. The body was cold and clammy, sometimes pouring out perspiration, and the hands and feet livid and shrivelled; the eyes shrunk and congested; the face haggard; the voice a husky whisper; dull stupor rapidly supervened, but when roused, the mind remained clear. The pulse was feeble and low, and soon imperceptible at the wrist. The blood was watery and dark; no bile nor urine was secreted. When the disease was speedily removed, there were generally no sequelae; in other cases, looseness continued, and, in a few, fever. In a few cases, the looseness was periodical, followed by most profuse perspirations. This is a complication with intermittent fever, which I have seen more prevalent in other parts of India, particularly in Mhow; and it is important, as it regulates the treatment, the most effectual part of which can only be advantageously used *before* the recurrence of the symptoms. In other cases, the state of collapse sometimes continued one or two days before warmth was restored to the surface, or urine or bile appeared. The watery evacuations continued, with pain in the centre of the abdomen, which continued after reaction ensued; the fever was of a typhoid character; the pulse was quick and feeble; the skin dusky, feeling like warm velvet; the liver and kidneys acted irregularly. The longer the period of collapse continued, the more marked were the typhoid symptoms, and the greater the emaciation on recovery. A troublesome diarrhoea was the most permanent of the sequelae.

The vomiting, purging, and cramps might be removed, and still the patient sink, unless the burning uneasiness in the praecordia subsided, or urine or bile appeared.

I examined two of the earliest cases, which proved fatal under twelve hours. In both the blood was watery and dark, the heart flaccid and empty, the lungs and intestines congested, the gall-bladder was filled with dark bile, and the bladder was empty. The state of the blood was similar to what I had observed in Paris in 1832, on the Ganges in 1833, and in different parts of India almost every year since then. Similar appearances were observed by Dr Anderson in August in the fatal cases amongst the 2d European regiment in cantonments. The appearances which I have on other occasions observed, in more protracted stages of the disease, in the mucous membrane of the intestines, and in the brain, were not invariable, and may be attributed to the suspended functions of the different organs; hence the effects, and not the cause of the disease.

The treatment was directed to check the symptoms, to promote the natural secretions, and to remove the sequelae of the disease.

Remedies that are easily obtained, and when application is plain and simple, even to unpractised people, are peculiarly valuable in this country, more especially when a rapidly fatal pestilence like Cholera rages extensively; when a few hours makes the difference between life and death, and where rational physicians are few and far between. The Cholera medicines here described were distributed through the town and district of Agra to each tannah or police station. They were placed under charge of a native doctor at Secundra, Tajand Rambag, where prisoners were located, and each Burkundaze in charge of working prisoners had a supply. There were about 20,000 pills distributed. The confidence of the people in their efficacy was very great. They were preferred to the medicine of the Hakeems and Baids, and the preference affected the income of these practitioners. Hence arose a report that the pills contained soap (made of hog's lard), which would destroy their caste. This was counteracted by a public intimation of the ingredients from the Kotwall. In Paris, and in other parts of Europe, the cry against the *government* measures for relief was *poison*. Here it was *caste*, the most spiritual, if not the most rational, of the two popular clamours.

On the appearance of the disease two of the Cholera pills[1] were given; and if vomited, one was repeated every half-hour, and a mustard poultice applied to the stomach. In the majority of cases, if taken early, no further treatment was required. On the following morning a small dose of calomel or blue pill, followed by castor-oil, was generally given, and mild farinaceous diet enjoined for several days.

In cases where collapse had supervened, or there was great prostration, the Cholera mixture[2] was given in doses from one to two ounces. It is of the same nature as the Cholera pills, only liquid and more active, and in some cases it appeared more efficacious. In some of the most intense cases of collapse a solution of one ounce of salt and half a drachm of carbonate of soda in a bottle of water was used, and reaction in some hopeless cases followed. Hot saline enemata were used with Serjeant M'Kay, but without benefit, though on former occasions I have found this the most powerful agent in inducing reaction. This remedy is not adapted to general use in India. A little brandy was given in sago with advantage, in low cases. Some of the collapsed patients expressed a great desire for sugar and water, and it was given copiously, and in some instances with decided benefit. Constant warm frictions with oil and turpentine, and hot applications, were employed with great relief to the cramps.

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[1] *Cholera Pills.*

Aphiun. Kala Mircha.
Opium, one part; black pepper, two parts.
Hing.
Assafoetida, three parts, well mixed and divided into five grain pills.

Cholera Mixture.

Opium, 20 oz.; Cayenne pepper, 40 gr.; assafoetida, 1 dr.; ammonia, 1 oz.; camphor mixture, 20 oz.

Small bleeding to the extent of four to eight ounces was employed towards the termination of the attack by Dr Walker; this did not prevent collapse supervening, nor did the cases recover more rapidly. When local inflammation appears, or reaction, local depletion is very beneficial; but I have on former occasions, as well as now, observed fatal collapse immediately follow the use of depletion in the *earlier stages*, and I have not observed any subsequent benefit to recommend the use of so dangerous a remedy. The treatment in the secondary stage, when reaction had followed long-continued collapse, and typhoid febrile symptoms had supervened, consisted in promoting the natural secretions and supporting the strength; and where the pain in the abdomen was severe, local depletion and counter-irritation, the remedies most efficacious were a combination of calomel, quinine, and assafoetida, with castor-oil, followed during convalescence by a bitter tonic, consisting of an infusion of cheryetta, aolah, and senna. The diet was milk and sago or rice. Diarrhoea was a common and often obstinate sequela. I have seen it last for years. Its pathology is obscure; there is not always inflammation of the mucous coat of the intestines. The liver is generally small, and of the usual anaemic appearance, found with great emaciation. The kidneys presented a similar flabby appearance in some cases, though Bright's disease is not common. Many of the cases depend on irregular action of the pancreas. These observations are founded on previous experience amongst Europeans. The best treatment consisted of mild alteratives, blisters, with change of climate and sea-air; very strict attention to diet is necessary; milk and farinaceous diet appears best; and rich highly-seasoned food with vegetables the worst. The remedies used with most advantage with the prisoners were a few leeches followed by blisters to the epigastrium, with one of the following pills (See Note[1]), morning and evening accompanied by Bhail Ghera Sherbet, and milk diet. The use of very nutritious food, to satisfy the cravings of a morbid appetite, or rapidly restore strength, and the use of astringents, are generally followed in a few days by an aggravation of the symptoms.

[1] ℞ Pulv. Ipecac, Pil. Hydrarg, Assafoetida, Ext. Gentianae, Ext. Aloes ā ā gr. xii, ft. Pil. xii.

From the accompanying tables, it appears that from a strength of 2,939 prisoners and 600 guards there were 264, or 7·46 per cent. attacked with Cholera, of whom 80 or 30·30 per cent. died; this is equal to 2·26 per cent. on the strength of the jail. There were 101 admissions during the night, and 163 during the day; of these (Table No. 5) there were 132 slight cases, of whom 10 or 7·57 per cent. died; 42 severe cases, of whom 12 or 27·90 per cent. died, and 89 collapsed cases, of whom 58 or 63·16 per cent. died. Of the 80 fatal cases, 51 or 62·88 died within 24 hours, of whom 4 were under 6, and 22 under 12 hours. There were only 14 or 1·75 per cent. died after the second day. From Table No. 3 it does not appear that the difference of age between 20 and 60 has much influence on the mortality. Beyond 60 the mortality is very high. Under 20 the number of cases is too small to afford a useful criterion. From Table No. 4 it appears that the mortality is high in proportion to the period of imprisonment. There is also a similar increase, though in a less degree, in the proportion of deaths to the number attacked. This difference depended on those imprisoned for short terms having been sent to the different outgangs, either before or after the Cholera broke out, whilst the life prisoners and bad characters were retained in the jail.

Table No. 2 shews the localities in which the disease appeared. The four central wards in the jail were most free from attack. They are old, low arch-roofed buildings, and less freely ventilated than the other more recent wards. The cases which appeared in the hospital amongst the patients or attendants were most fatal.

JOHN MURRAY, M.D. *Civil Surgeon, Agra.*

Agra, Nov. 1, 1851.

P.S.—During the year 1852, the Cholera was very prevalent and fatal at the foot of the Himalayan Hills, in St Babur, and at Deyrah Dhoon, and also at Umballah. At the same rate it should appear at Lahore in 1853.

J. M.

No. I.—STATISTICAL TABLE *of the Cases of* CHOLERA *in the Central Jail at Agra in the year* 1851.

DATE.	STRENGTH.			ADMISSION.			Deaths.	Recoveries.	RESULT OF THE ADMISSIONS OF EACH DAY.															PERIOD OF IMPRISONMENT.					Guard.	Fall of Rain in inches.
1851.	In Jail.	In Out-gangs.	Total.	During Night.	During Day.	Total.			Died.	Under 6 hours.	Under 12 hours.	Under 1 Day.	Under 2 Days.	Under 4 Days.	Under 8 Days.	Over 8 Days.	Re-covered.	Under 1 Day.	Under 2 Days.	Under 4 Days.	Under 8 Days.	Under 12 Days.	Over 12 Days.	Under 3 Years.	Under 7 Years.	Under 14 Years.	Under 21 Years.	For Life.		
July 25.	1882	1057	2939	...	1	1	...	...	1	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	0·40
26.	1888	1053	2941	...	1	1	1	...	...	...	..	...	...	...	...	...	1	...	...	...	1	...	...	...	...	1	...	...	...	0·23
27.	1871	1066	2937	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	0·24
28.	1856	1070	2926	1	2	3	1	...	3	...	1	1	1	...	...	...	...	...	...	...	...	...	...	...	...	1	...	2	...	...
29.	1841	1082	2928	...	4	4	2	3	...	...	...	...	...	...	...	...	4	3	...	...	1	...	...	...	1	1	...	1	1	...
30.	1841	1080	2921	1	2	3	...	1	2	...	...	...	2	...	...	...	1	...	1	...	...	...	...	...	3	...	...	...	...	...
31.	1680	1224	2920	9	6	15	2	...	6	1	2	3	...	...	...	...	9	2	...	1	5	1	...	1	1	5	1	7	...	...
Aug. 1.	1556	1849	2905	6	10	16	8	4	4	1	1	1	...	...	1	...	12	4	3	4	1	...	...	2	3	6	...	5	...	0·29
2.	1548	1361	2909	10	7	17	2	7	4	...	1	1	1	...	1	...	13	...	4	5	1	1	2	...	4	1	...	11	1	...
3.	1553	1346	2899	5	3	8	3	...	4	...	3	...	...	...	1	...	4	1	...	3	...	...	...	...	1	4	...	3	...	...
4.	1548	1344	2892	5	12	17	3	14	8	...	2	5	...	...	1	...	9	...	6	1	1	...	1	1	2	2	1	9	2	1.62
5.	1546	1340	2886	4	15	19	9	2	7	...	2	2	1	1	1	...	12	2	...	1	6	2	1	1	6	3	1	8	...	1·45
6.	1544	1335	2879	6	10	16	4	16	4	...	1	1	1	...	...	1	12	2	2	6	1	1	...	1	7	4	...	3	1	1·05
7.	1545	1331	2876	6	8	14	1	5	2	...	2	...	...	...	...	...	12	...	1	6	3	1	1	...	5	4	...	4	1	2·90
8.	1548	1324	2867	5	13	18	4	3	5	...	...	2	3	...	...	...	13	4	4	2	3	...	...	1	3	7	1	6	...	1·43
9.	1527	1330	2857	9	8	17	8	13	6	...	1	4	1	...	...	...	11	2	2	3	3	1	...	2	1	3	1	7	3	0·10
10.	1585	1311	2846	4	9	13	6	16	2	...	2	...	...	...	...	...	11	2	...	3	3	2	1	1	2	4	2	4	...	...
11.	1531	1310	2841	7	9	16	4	8	5	...	1	...	1	...	2	1	11	...	4	3	3	...	1	1	5	4	1	5	...	...
12.	1517	1314	2831	6	5	11	5	...	5	...	2	2	1	...	...	...	6	...	1	4	...	1	...	1	5	3	...	2	...	...
13.	1528	1303	2831	5	5	10	...	17	2	...	...	...	2	...	...	...	8	...	1	1	3	2	1	1	5	2	...	2	...	...
14.	1549	1295	2844	4	5	9	3	5	2	...	...	...	...	2	...	...	7	...	1	1	3	1	1	...	...	4	2	3	...	...
15.	1550	1290	2840	...	4	4	1	5	1	...	...	1	...	...	...	...	3	...	...	1	2	...	...	...	1	...	...	2	1	...
16.	1553	1284	2835	1	3	4	2	18	...	...	...	...	...	...	...	...	4	...	..	2	1	1	...	...	...	3	...	1	...	...
17.	1570	1265	2835	...	1	1	...	3	...	...	...	...	...	...	...	...	1	...	...	...	1	...	...	...	...	1	...	...	...	...
18.	1562	1268	2825	...	1	1	1	5	1	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...
19.	1568	1253	2821	...	4	4	3	...	1	1	...	...	...	...	...	...	3	...	...	1	1	1	...	1	2	1	...	...	...	...
20.	1672	1143	2815	...	5	5	...	7	...	..	...	...	...	...	...	...	5	...	4	1	...	...	...	1	...	2	...	1	1	...
21.	1688	1136	2824	...	1	1	...	4	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...
22.	1691	1128	2819	...	1	1	1	6	...	...	...	...	...	...	...	...	1	...	...	1	...	...	...	...	1	...	...	...	...	...
23.	1665	1158	2818	2	2	4	...	1	1	...	...	1	...	...	...	...	3	1	...	1	..	...	1	...	1	1	...	2	...	...
24.	1766	1047	2813	...	2	2	...	8	2	...	...	...	...	1	1	...	...	...	...	...	...	...	...	...	...	1	...	1	...	...
25.	1734	1080	2814	1	...	1	...	...	...	...	...	...	...	...	...	...	1	...	..	1	..	...	...	...	...	1	...	...	...	...
26.	1734	1078	2812	3	...	3	1	1	...	...	...	...	...	...	...	...	3	...	3	...	...	...	...	...	3	...	...	...	...	...
27.	1735	1075	2810	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	~	...	...	...	...	...
28.	1739	1168	2807	...	...	...	1	4	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
29.	1749	1059	2808	1	3	4	1	2	1	1	...	...	...	...	...	...	3	...	1	...	2	...	...	...	3	...	...	1	...	...
30.	1757	1051	2808	...	1	1	...	...	...	...	...	...	...	...	...	...	1	...	...	...	1	...	...	...	1	...	...	...	...	...
31.	1764	1040	2804	...	...	...	1	5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1·58
Total.	...	...	...	101	153	264	80	184	80	4	22	25	15	4	8	2	184	23	38	52	46	15	10	15	67	69	10	92	11	10·24

No. II.—*Locality in which the Cholera appeared.*

	No. of Wards in Jail.																									
	2	3	4	5	6	7	8	9	10	14	15	19	20	21	28	Solitary cells.	Hospital.	Convalescent.	Work.	Garden.	Drain.	Outgangs.	Secundra.	Taj.	Guard.	Total.
Strength,	70	70	70	74	121	88	80	63	70	79	90	90	90	90	90	73							309	197	600	
Admission,	2	4	8	9	21	11	5	10	20	1	2	3	6	7	1	13	12	4	31	17	12	24	17	13	11	264
Deaths,	...	...	4	...	4	5	4	4	2	1	1	3	2	2	...	1	9	1	6	9	1	3	11	2	5	80

No. III.—*Ages.*

	Under 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	Over 60.	Total.
Admissions, . . .	3	87	92	39	25	18	264
Recoveries, . . .	3	65	67	24	15	10	184
Deaths, ...	...	22	25	15	5	8	80
Ratio per cent. deaths to admission, .	...	25·28	27·17	38·46	20·00	44·44	30·30

No. IV.—*Period of Imprisonment.*

		Under 3 years	3 to 7.	7 to 14.	14 to 21.	For Life.	Jail Guard.	Total.
No. of prisoners and guard, . . .		435	997	866	85	556	200 N. 400 B.	3539
Admissions, . . .		15	67	69	10	92	11	264
Recoveries, . . .		12	49	54	7	56	6	184
Deaths, ...		3	18	15	3	36	5	80
Rates per cent.	Admissions to strength, .	3·45	6·72	7·96	11·76	16·56	1·83	7·46
	Deaths to admissions, . .	20·00	26·86	21·74	30·00	39·13	45·45	30·00
	Deaths to strength,	0·69	1·80	1·73	3·53	6·47	0·83	2·26

No. V.—*Intensity of the Disease on Admission into Hospital.*

	Slight.	Severe.	Col-lapsed.	Total.
Admissions, ...	132	43	89	264
Recoveries, ...	122	31	31	184
Deaths, ...	10	12	58	80
Ratio per cent. deaths to admissions,	7·57	27·90	65·16	30·00

No. VI. — *Comparative Statement exhibiting the Average Total Deaths reported in the City of Agra and Environs, for the Months of August and September* 1849–50, *as compared with Total Deaths in* 1851.

	AUGUST.						SEPTEMBER.						
Date.	No. of Deaths for 1849.	No. of Deaths in 1850.	Average.	No. of Deaths in 1851.	Decrease.	Increase.	No. of Deaths for 1849.	No. of Deaths for 1850.	Average.	No. of Deaths in 1851.	Decrease.	Increase.	Date.
1	11	9	10	8	2	...	8	5	6	34	..	28	1
2	12	4	8	6	2	...	16	9	13	25	...	12	2
3	16	3	9	10	...	1	9	3	6	19	...	13	3
4	12	6	9	5	4	...	14	3	9	29	...	20	4
5	7	2	5	12	...	7	13	8	10	13	...	3	5
6	9	20	14	12	2	...	15	14	14	25	...	11	6
7	4	12	8	8	...	...	10	14	12	13	...	1	7
8	12	10	11	12	...	1	4	3	4	17	...	13	8
9	13	4	9	16	...	7	10	7	9	10	...	1	9
10	4	6	5	13	...	8·24	7	16	12	18	...	6.108	10
11	9	4	7	11	...	4	7	11	9	11	...	2	11
12	2	2	2	14	...	12	12	4	8	12	...	4	12
13	2	11	6	20	...	14	17	13	15	17	...	2	13
14	7	5	6	24	...	18	10	12	11	9	2	...	14
15	11	2	7	11	...	4	15	11	13	13	...	...	15
16	14	2	8	16	...	8	10	9	9	9	...	...	16
17	9	5	7	17	...	10	17	8	13	11	2	...	17
18	15	4	9	13	...	4	24	11	18	8	10	...	18
19	5	4	5	16	...	11	22	9	15	10	5	...	19
20	5	13	9	13	...	4·89	9	16	12	5	7	·8	20
21	12	7	10	28	...	18	4	7	6	9	...	3	21
22	10	6	8	24	...	16	7	8	7	5	2	...	22
23	14	4	9	20	...	11	8	9	9	10	...	1	23
24	7	10	8	18	...	10	8	13	10	14	...	4	24
25	12	8	10	9	1	...	16	11	14	10	4	...	25
26	11	8	10	33	...	23	8	13	10	9	1	...	26
27	17	4	10	29	...	19	9	7	8	4	4	...	27
28	7	7	7	20	...	13	18	12	15	14	1	...	28
29	6	5	6	32	...	26	9	16	12	5	7	...	29
30	9	7	8	27	...	19	21	13	17	10	7·52	...	30
31	15	5	10	13	...	3	...	...	...	...	...	...	...
Total	299	199	249	510	11	271	357	295	326	398	52	1·24	

PART II.

CRITICAL ANALYSIS.

ART. I.—*Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England.* BY JAMES PAGET, F.R.S., lately Professor of Anatomy and Surgery to the College; Assistant Surgeon and Lecturer on Physiology at St Bartholomew's Hospital. Volume I. HYPERTROPHY; ATROPHY; REPAIR; INFLAMMATION; MORTIFICATION; SPECIFIC DISEASES. London, 1853. 8vo, pp. 499. Volume II. TUMOURS. London, 1853. 8vo, pp. 637.

Some doubt may justly be entertained whether the term SURGICAL PATHOLOGY be quite logical and philosophical, either in its origin or in its application; and whether the distinction which it implies is truly founded on scientific and essential differences. Pathology, it may be said, is one science, whether it relates to medicine or to surgery. The changes which it enumerates and describes, and the lesions of which it narrates the history, all agree in being departures from the healthy structure and organization of the tissues and organs, of which the body consists. The distinction of Pathology into medical and surgical, relates only to treatment, and to the division of the profession into physicians and surgeons. The same changes in structure which affect the skin, the extremities, and other external parts, may affect the lungs, the heart, the stomach, and the liver. All the tissues which compose these internal parts and internal organs, are liable to undergo the same morbid changes; and it is unphilosophical, it may be argued, to found distinctions on accidental, or at least indifferent characters, like those of mere position and situation.

All this is doubtless true, and a good deal more might be said to shew, that it must be true. But again, though the distinction cannot be said to be philosophical or logical, it is at least convenient ; it is sanctioned by a sort of practical usage; and it possesses many advantages, which recommend it at once to teacher, to pupil, and to practitioner. A certain class of diseases affecting the internal parts, the outlets, and the extremities of the animal body, it has long been the province of the surgeon to treat. Almost all these diseases were, it cannot be concealed, treated by local applications, by various instrumental means, by operations of cutting, or removal by the knife or scissors, of mutilation, ablation, and excision of various sorts. Though the limits of this mechanical surgery, as it may be called, have undergone a considerable degree of contraction, the diseases have still remained to be treated by surgeons upon more extensive and more just principles. The example and authority of such men as John Hunter, John Abernethy, Mr Lawrence, Mr Travers, Sir Astley Cooper, Mr Vincent, and Sir Benjamin Brodie, have exerted a most beneficial influence in shewing the absolute necessity of the extensive and profound study of Physiology and Pathology, of healthy actions, and morbid actions and structures, in regulating and applying the treatment of surgical diseases. By considering these diseases as not local affections only, but as manifestations of particular states of the system at large, as individual but integral parts of one great whole, it is virtually admitted, that Pathology, as a science or branch of medical knowledge, recognizes not the distinction into Medical and Surgical.

Considered in this light, the distinction between Medical and Surgical Diseases, or between General and Local Diseases, becomes entirely arbitrary and artificial. In point of fact, though Cullen and various other nosologists have made a class LOCALES and TOPICI, speaking according to strict physiological and pathological principles, there is no such thing in existence as a local disease. One disease may be more local and less general than another; but the existence of an affection absolutely local, that is, entirely independent of the organs, the actions and the functions of the general organism, and of the laws to which the organism is subject, is altogether a creation of the fancy. If any affection appears at first sight to be local, it is in no long time found to be the product of some change in the system at large; and though any given affection may seem at first sight to be local, it speedily shews, that it exerts upon the constitution at large very decided and perceptible influences.

It must be granted, therefore, that the distinction of Pathology into Medical and Surgical is arbitrary and artificial, and may be very well suited for communicating information of a certain kind and form, but is at variance with the natural and essential characters of the subject, and is calculated to convey, if not to impress, erroneous ideas on the true nature and objects of Pathological Science.

The justice of these remarks appears to us to be so much more forcible, when it is remembered that the Royal College of Surgeons in England itself, have, in the establishment of their extensive collection, and in the constant attention which they have bestowed upon its enlargement, not ambiguously approved the principle that Pathology, or the doctrine of morbid actions and structures, is a science, one, and indivisible, and that which is styled Surgical Pathology, is as much a part of Medicine as of Surgery. Even in a therapeutic point of view, the distinction is not by any means in all instances tenable. Every experienced surgeon knows that many cases commonly called surgical, he must treat upon general medical principles; and that though operation and local management may be often required, yet it is unsafe and unprofitable to disregard the medical treatment of such maladies.

Though we feel it to be our duty to submit these remarks, we allow that the distinction of Pathology into Medical and Surgical is convenient, and one which we are obliged, in certain circumstances, ourselves to adopt. All that we here mean to inculcate is, that we are quite aware, that it is not perfectly philosophical and logical; and having said this much, we proceed to introduce the work of Mr Paget to our readers.

The greater part of the Lectures contained in these volumes was delivered at the Royal College of Surgeons during the six years intervening between 1847 and the end of 1852, in which the author held the office of Professor of Anatomy and Surgery to the College. The substance of several of them has been in various modes before the public. But in the present form they have received that complete degree of revision and rectification, of detail and information, which the author hopes may render them more perfect as a body of pathological and practical knowledge, and more serviceable to those who are desirous to render their knowledge of Pathology exact, accurate, and extensive.

The first volume contains twenty lectures, of which the subjects are the following.

I. Nutrition; its nature, purpose, and conditions. II. The conditions necessary to healthy nutrition. III. The Formative process; Growth. IV. Hypertrophy. V. Atrophy; Degeneration. VI. Atrophy. VII. General considerations on Repair and Reproduction. VIII. The Materials for the Repair of Injuries. IX. and X. The process of the Repair of Wounds. XI. The Repair of Fractures. XII. The Repairs of Injuries in various Tissues. XIII. The Phenomena of Inflammation. XIV. Products of Inflammation. XV. Developments of Lymph. XVI. Degenerated Forms of Lymph. XVII. Changes produced by Inflammation on the affected parts. XVIII. Nature and Causes of Inflammation. XIX. Mortification. XX. Specific Diseases.

The Second Volume is devoted exclusively to the extensive and complicated subject of Tumours, or New and Heterologous Growths. The lectures are in number sixteen; and in them the author treats of the following subjects.

I. The Classification of Tumours; II. and III. Cysts of various kinds; IV. Fatty and Fibro-cellular Tumours; Painful Subcutaneous Tumours; V. Fibrous Tumours; VI. Recurring Fibroid and Fibro-nucleated Tumours; VII. Cartilaginous Tumours; VIII. Myeloid and Osseous Tumours; IX. Glandular Tumours, and Vascular and Erectile Tumours. X. Skirrhous or Hard Cancer; XI. Medullary or Soft, Spongy Cancer; XII. Epithelial Cancer; XIII. Meanoid, Haematoid, Osteoid, Villous, and Colloid Cancers; XIV. and XV. General Pathology of Cancer, and Conditions giving rise to the formation; and XVI. Tubercle.

Mr Paget begins his Lectures by considering the process or function of Nutrition, and explaining its nature, purpose, and the conditions necessary to its normal performance. These conditions are four: 1*st*, a right state and normal composition of the blood or other nutritive material; 2*d*, a regular and not far-distant supply of such blood; 3*d*, at least in most cases, a certain degree of influence of the nervous system; and, 4*th*, a natural state of the part to be maintained.

In speaking of the first condition, he dwells a little upon the principle of symmetry, as exemplified in the phenomena of disease; that is to say, the affection of like or the same parts on each side of the mesial plane of the animal body. Thus it is known that in chronic rheumatism, or chronic arthritis, when one joint becomes affected, the corresponding one is, in the majority of cases, affected in nearly the same manner. In all these instances, argues Mr Paget, it must be inferred, that the morbid substance in the blood, whatever it may be, acts upon and changes only certain portions of what we might suppose to be all the very same tissue. Such a substance fastens on certain islands, says Mr Paget, on certain surfaces of two bones, or of two parts of the skin, and leaves the rest unscathed; and these islands are the exactly corresponding pieces upon opposite sides of the body. The conclusion, he continues, is unavoidable, that these are the only two pieces that are exactly alike; that there is less affinity between the morbid material and the osseous tissue, or the skin, or the cartilage close by; else it would have been similarly diseased.

This subject of the conditions necessary to healthy nutrition is very fully examined in the second Lecture.

In the third Lecture after giving some judicious explanations of the process of growths, he enters on the consideration of Hypertrophy; and under this form he includes only those cases (p. 66) in which the enlargement of a part is effected with development and increase of its natural tissue, with retention of its natural form, and with increase of pain. The conditions necessary to hypertrophy are chiefly in only three, namely;

1. The increased exercise of a part in its healthy functions; 2. An increased accumulation in the blood of the particular materials which a part appropriates to its nutrition, or in secretion; and, 3. An increased afflux of healthy blood.

He then examines in the fourth Lecture, Hypertrophy of Muscle and Hypertrophy of Bone. Of the latter the following description is given.

"Hypertrophy of bone presents itself in many interesting cases. It is usually a secondary process, ensuing in consequence of change in a part with which some bone is intimately connected. Just as in their natural development and growth, the bones of the skull are formed in adaptation to the brain, and those of the limbs are framed to fitness for the action of the muscles; so, in disease, they submit in their nutrition to adapt themselves to the more active parts. Thus, the skull enlarges when its contents do; and the bones of the limbs strengthen themselves as the muscles inserted on them become stronger and more active; and they do this in adaptation to the force of the muscles, and not merely because of the movements they are subject to: for no extent or force of passive movement would prevent the bones of a limb whose muscles are paralyzed from suffering atrophy.

"In the skull, if in any organ, we might speak of two forms of hypertrophy; eccentric and concentric. When the cranial contents are enlarged, the skull is hypertrophied with corresponding augmentation of its area; and when the cranial contents are diminished, the skull (at least in many cases) is also hypertrophied, but with concentric growth, and diminution of its capacity.

" The first, or eccentric form, is usually the consequence of hydrocephalus; wherein, as the fluid collects and distends the dura mater, so the skull grows; still, as it were, striving to attain its purpose, and form a complete envelope for the expanding brain.

" The process of enlargement in these cases is often one of simple growth, and that, indeed, to a less extent than it may seem at first sight: for it is very rarely that the due thickness of the skull is attained while its bones are engaged in the extension of their superficial area. Hence, the weight of an hydrocephalic skull is not much, if at all, greater than that of a healthy one ; a large parietal bone,[1] measuring nine inches diagonally, weighs only four ounces, while the weight of an ordinary parietal bone is about three ounces.

" It is interesting to observe, in some of these cases, the symmetrical placing of the Wormian bones, by which the extent of the skull is in a measure made up. They shew how the formative process, though thus thrown into straits and difficulties, yet conforms, both in growth and development, with the law of symmetry.

" It would be yet more interesting if we could certainly trace here something of conformity with the law of unity of organic type, in the mode of insertion of these Wormian intercalary bones, when compared with those of other animals. It cannot be certainly done; and yet, in some of these specimens, there appears (as if in accordance with that law) a tendency to the formation of the Wormian bones at the posterior part of the sagittal suture more than in any other part, as if in imitation of the interparietal bones of Rodents. And in the very rare specimen,[2] in the midst of great confusion of the other bones, we find a remarkable bony arch, extending from between the two frontals to the occipital bone; occupying, therefore, the place of a large interparietal bone, and reminding us of some of the monkeys, *e. g.*, Cebus and Jacchus. We have a somewhat corroborative specimen in the immense hydrocephalic skull of the skeleton from Mr Liston's Museum (No. 3489), in which the interparietal Wormian bones are larger than any others.

" The hypertrophy of the skull, which may be called concentric, is that which attends atrophy with shrinking of the brain, or, perhaps, any disease of the brain in which there is diminution of its bulk. In such a case it usually happens, as was first shewn by Dr Sims,[3] that the skull becomes very thick.

" All the specimens which I have examined shew, however, that in these cases the thickening of the skull is not, in itself, a morbid process; it manifests definite purpose; is usually effected by healthy growth; and observes the rules followed in the natural formation of the skull.

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[1] No. 2 in the College Museum.

[2] No. 3487 in the same Museum.

[3] Medico-Chirurgical Transactions, vol. xix., p. 315.

"Thus, as in first formation, the skull adapts itself to the form and size of the brain, or, rather, of its membranes; only now it does so without representing on its exterior the change which has taken place within. The thickening of the skull is effected by the gradual remodelling of the inner table and diploe of the bones of the vault; so that, although the exterior of the skull may retain its natural form and size, the inner table grows more and more inwards, as if sinking towards the retiring and shrinking brain; not thickening, but simply removing from the outer table, and leaving a wider space filled with healthy diploe.

"Again, it is a fact of singular interest, that this thickening, this hypertrophy of the skull, most commonly, if not always, takes place especially, and to a greater extent than elsewhere, in the parts of the bones in and about which ossification commenced in the foetal state: as if, one might say, some of the potency that of old brought the foetal membrane of these parts first into the development of bone, were always afterwards concentrated in them; or as if a reserve power of growth had its seat in the same centres where was formerly the originative power of development. The fact is shewn in many cases of the specimens; and we may find some further, though less sure, evidence of the peculiar formative energy of these old centres, in the fact that those diseases of bone which are accompanied with excessive formation, such as morbid thickenings of the skull and tumours, are, in a large majority of cases, seated in or near the centres of ossification; you rarely find them except at the articular ends, or round the middle of the shaft. The same does not hold of nekrosis, rickets, ulceration, or other diseases indicative of depression of the formative power of the bone. Rather, as some specimens (Nos. 390-1-2) of rickety disease of the skull and femora shew, the centres of ossification are remarkably exempt from the change of structure which has extensively affected the later-formed parts.

"This peculiarity of the centres of ossification is the more remarkable when we remember that, in many cases, the thickening of the skull takes place in persons far past the middle period of life; it may happen even in very old age, and may give one more evidence of that precision of assimilation which maintains, throughout life, characteristic distinctions among portions of what we call the same tissue."—(Pp. 79–82.)

An effect not uncommon of hypertrophy affecting the long bones, is their elongation. This is most usually seen in the the Femur, and sometimes in the Tibia; and when it takes place in the latter bone, Mr Paget states that it causes curvature of the Tibia. In other words, certain forms of curvature of the Tibia, Mr Paget ascribes to the previous existence of Hypertrophy of the bone; and lest this should be confounded with rickety curvature, he adds that the rickety Tibia is always short ; whereas the Hypertrophied Tibia, though incurvated, is longer than the normal standard.

> " The elongation of the bones in these cases may occur, in different instances, in two ways. In some cases it seems due to that change in bone which is analogous to chronic inflammation of soft parts, and which consists in the deposit of the products of inflammation in the interstices of the osseous tissue, their accumulation therein, and the remodelling of the bone around them as they accumulate. Such a change appears to have occurred in the specimen from which the sketch was taken, and would necessarily give rise, in a growing bone, as it does in soft parts, to enlargement in every direction, to elongation as well as increase of circumference.
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> " But, in other cases, the elongation is probably due to the more genuine hypertrophy which follows the increased flow of blood. When, for example, a small portion of bone, as in circumscribed nekrosis, is actively diseased, all the adjacent part is more vascular ; hence may arise a genuine hypertrophy, such as I have shewn in hair under similar circumstances."—(P. 86.)

Atrophy, to which the author assigns the synonymous epithet of Degeneration, forms the subject of the fifth and sixth Lectures.

By Atrophy, Mr Paget informs his auditors, is commonly implied not the cessation or total privation of the formative process in a part, but its Deficiency ; and as he limits Hypertrophy to the cases in which increased power is acquired for a part by the growth or by the development of healthy tissue ; so Atrophy is taken to designate only that process by which a part either simply wastes and is reduced in size, with little or no change of texture, or else gradually and regularly degenerates.

By the terms of this limitation it is implied that, as there are two modes of Hypertrophy, the one with growth, the other with development, so there are two modes of Atrophy, the one with simple decrease, the other with degeneration of tissue. In both there is loss of functional power in the part ; but in the one this loss is due to the deficient quantity, in the other to the deteriorated quantity of the tissue. The author subsequently explains at greater length his ideas on the nature of Degeneration.

We feel little disposed to criticise closely the distinctions and reasonings of one, who like the present author, has evidently been at great pains and undergone much labour, in order to enable him to present correct views on the several divisions of his subject. But it appears to us, that there is in the mode of considering Atrophy now mentioned something confused and not quite logical. Atrophy is, we conceive, one lesion; Degeneration is another. Atrophy, according to the admission of Mr Paget himself, is diminished or undernutrition, not privation of nutrition in a part; that is, it is Hypotrophy. When a muscle becomes atrophied, we understand that the individual, integrant, and component fibres, become small, shrunk, and pale, yet not internally changed. All the fibres are there; but they are diminished in size and bulk by inanition. In Degeneration, as we understand it, these fibres are entirely changed; nor can we imagine Degeneration without a change in the substance of these fibrils. As to the fashion or mode, whatever it may be, of calling a muscle which has become fatty, an atrophied muscle; the expression is to us altogether unintelligible. If fat or oil is substituted for the muscular fibres, which is the fact, then there is a positive and decided change. The change produces perhaps atrophy, and the muscle is weakened; but surely when for one normal substance is substituted another totally different, the change is much greater than when the fibres are there, and only more or less diminished in size. So is it with the fatty transformation in general.

The main cause of all this confusion in thinking and speaking, is the fact that the word Atrophy is employed to designate a considerable number of changes in textures, which changes are totally different from each other. We must, however, allow Mr Paget to give his own views of the subject.

Mr Paget enumerates five different forms of Atrophy.

> " 1. Wasting or withering; the latter term may imply the usually coincident wasting and drying which constitute the emaciation of a tissue. 2. Fatty degeneration, including many of what have been called granular degenerations. 3. Earthy degeneration, or calcification. 4. Pigmental degeneration. 5. Thickening of primary membranes."—(P. 98.)

Of all these changes, Fatty Degeneration is the one which has been most attentively observed and most fully examined. The following passage presents a good general view of the leading characters of this transformation.

> " Their principle general feature is, that in the place of the proper substance of an elemental structure, *e. g.*, in the place of the contents or the nucleus of a cell, or in the very substance of a simple membrane, a blastema, or a fibre, minute particles or granules are seen, which are recognised as consisting of oily or fatty matter, by their

peculiar refraction of light, their solubility in ether, their aptness to coalesce into larger oil-drops, and, when they are very abundant, by the greasiness of the whole tissue, its burning with a bright flame, and its yielding to analysis an unusual quantity of fatty matter. In examining organs in the state of fatty degeneration, we may commonly see the progress of the change in the gradual increase of the fatty particles. Some cells, for example, may appear quite healthy; some may deviate from health only in containing two or three shining, black-bordered oil-particles; in others, these are increased, and a large part of the cell-cavity is filled with minute oil-particles, or with one or more larger oil-drops; and in others, the contents of the cell have given place to a single cluster of oil-drops. In this last case, the degeneration is nearly complete: the transformed cell is called a 'granule cell,' or, when, as it often happens, the cell-wall has wasted and disappeared, it is a 'granule mass;' and the last stage of degeneration is that such masses may break up, their constituent molecules may dispart, and the tissue which was an aggregate of nucleated cells may become little more than a mass of molecules or drops of oily matter.

"It is probably due in part to such disintegration of degenerate cells, that, in most organs thus degenerate, abundant fatty matter is found free, that is, lying in drops not inclosed, among the proper constituents of the tissue. But this free fat is also derived, in part, from the degeneration of inter-cellular substance, which is usually concurrent with that ensuing in the cells; and in some cases (as Virchow has observed in the liver) it so follows the arrangement of minute bloodvessels that it may be considered as the residue of a direct deposit or exudation from them.

"In most instances the fatty degeneration affects, first and chiefly, as I have described it, the contents of cells or tubules, or the proper substance of membrane or other tissue. And when it thus happens, the nuclei almost always waste, and either shrivel or disappear after gradually fading in their outlines. This may be commonly seen in the fatty degeneration of the renal and hepatic cells, and of the muscular fibres; and it is a fact of some significance, when we remember the constancy and abundance of nuclei in actively-growing parts. But, in certain cases, as in fatty degeneration of cartillages, the change appears to begin in the nuclei, which are gradually transformed into granule masses, while the cell-wall may remain unchanged, or may become thickly walled or laminated, or may coalesce with the surrounding tissue.

"Such a transformation of nucleus, while it retains its place and general form, might at once suggest that the fatty matter which collects in these degenerations is not introduced from without into the cells or other elements of the tissues; that it is not placed in them, as it may be in the parts around them, as a morbid deposit, or exudation from the bloodvessels; but rather is one of the products and residues of some chemical transformation which they undergo when the proper nutritive changes are suspended. We might derive the same suggestion from the similarly degenerate muscular fibres; in which we may often find the fat particles arranged in the same manner as the proper constituents of the fibrils, and looking as if there were a gradual transformation of the 'sarcous elements' into the little oily particles, which, by clustering, and then by fusion, at length compose the larger oil-drops."—(Pp. 104–106.)

As, however, the subject has been very fully placed before the readers of this Journal, by the instructive papers of our correspondent Mr Hallett, and the comprehensive essay of Dr Richard Quain, we must be satisfied with a general reference to the lectures of the present author.

The singular disease known under the name of *Mollities Ossium*, Mr Paget regards as a fatty degeneration of the bones. The grounds upon which he founds this opinion are, first, the general fact recognized by anatomists, that the softened matter is really fatty, and secondly the facts disclosed by microscopical examination of the diseased bones, which shews that the soft, reddish, pink-coloured matter, consists of free oil in great quantity; crystals of margarine free or inclosed in fat-cells; a few fat-cells full of oil as in health, but many more empty, collapsed, and rolled up in strange and unusual forms. The pink and crimson colours are owing to the bright tints of the oil-globules, and of the nuclei and granules in the collapsed fat-cells; and there is little appearance of any excess of blood in such bones or in their contents.

This is the form of diseased bone to which Mr Curling proposes to apply the name of Eccentric Atrophy of Bone, to express one of the principal characters of the disease. The author thinks that this appellation is preferable to that of *Osteoporosis*, as employed by Rokitansky; but he prefers giving it the name of Fatty Degeneration. The proper denomination, if this view be well founded, would be *Osteosteatosis*.

Mr Paget further thinks, that under the term *Mollities Ossium*, as used by practical observers, two diseases are included; one, the Fatty Degeneration exhibited by ordinary specimens of the disease, and the other the simple softening of bone, occasionally but not often seen and known as rickets in the adult; and of which the only probable well-recorded instance is that narrated by Dr Macintyre, to which the attention of the readers of this Journal was particularly directed in volume seventy-fourth, p. 357, October 1850, and volume seventy-fifth, p. 157, January 1851. If, however, we trust to the results of analysis, the latter was as much and as clearly an instance of fatty degeneration as any case could be. Oil-globules and fat cells in abundance were found in the red gelatiniform substance which filled the large cancellated cavities of the diseased bones. These, it will be remembered, were the ribs and the vertebrae. The bones of the extremities had not become affected.

The author allows that the nature of this change in the bones is still involved in doubt and obscurity.

The archbishop of Canterbury mentioned at p. 134 as forming the subject, in which the two *femora* were softened by this disease, and numbered 400 in the Hunterian Museum of the Royal College, was, we presume, Thomas Secker, who, after having laboured under this disease for several months, died of it on Wednesday the 3d of August 1768, in the seventy-fifth year of his age.[1]

The disease, which has been long known under the name of Steatomatous Degeneration of the Arteries, was several years since shewn by Mr Gulliver to consist in adipose or fatty degeneration of their tunics. The following description conveys a correct idea of the nature of this change.

"In the least degrees of this affection, the only apparent change of structure is, that minute, shining, black-edged particles, like molecules of oil,[2] are thinly and irregularly scattered beneath the outer surface of the small bloodvessels of the brain. Such a change may be seen in the vessels of portions of the brain that appear quite healthy, as well in the capillaries as in branches of both arteries and veins of all sizes, from 1-150th of an inch in diameter, to those of smallest dimension.

"As the disease makes progress, the oil-particles may increase in number till the whole extent of the affected vessels is thick-set with them, and the natural structures, even if not quite wasted, can hardly be discerned. While their number thus increases, there is, also, usually, a considerable increase of the size of many of the oil-particles, and they may be seen of every size, from an immeasurable minuteness to the diameter of 1-2000th of an inch. In other places one sees, instead of this increase of scattered oil-particles, or together with it, groups or clusters of similar minute particles, which are conglomerated, sometimes in regular oval or round masses, like large granule-cells, but more often in irregular masses or patches, in the wall of a great part of the circumference of a bloodvessel.

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[1] Life of Thomas Secker, archbishop of Canterbury, by Beilby Porteus, bishop of London ; Works, volume sixth. London, 1823.

[2] Dr Jenner (Med. Times and Gaz., Jan. 31, 1852) has shewn that these appearances of oil-particles are very closely imitated by equally minute particles similarly deposited, but which are proved to be calcareous, by their solubility in hydrochloric acid. I think it very probable that what I have here described as fatty or oily matter may often be, at least in part, calcareous ; we may reasonably expect this affection of the small vessels to be exactly analogous to the common fatty and calcareous degeneration of the larger arteries, although there is no generality of coincidence between them. I have also seen a pigmental degeneration of small cerebral arteries very similar to the fatty one described above.

"In a single fortunately selected specimen, one may see, in different branches of a vessel, all these degrees or states of the degeneration; the less and the more thickly scattered minute oil-particles, the clusters of such particles in various sizes and shapes, and the larger particles like drops of oil.

"When the degeneration has made much progress, changes in the structure, and, not rarely, changes in the shape also, of the affected bloodvessels may be observed. The chief change of structure appears to consist in a gradual wasting of the more developed proper structures of the vessels; growing fainter in apparently the same proportion as the disease makes progress, the various nuclei or fibres are at length altogether lost, and bloodvessels of even 1-150th of an inch in diameter appear like tubes of homogeneous pellucid membrane, thick set with the fatty particles. The structures of the vessels are not merely obscured by the abnormal deposits; they waste and totally disappear.

"The changes of shape which the vessels may at the same time undergo are various. Very commonly, the outer layer of the wall is lifted up by one or more clusters of oil-particles, and the outline of the vessel appears uneven, as if it were tuberous or knotted. Sometimes the outer or cellular coat of the vessels is for some distance raised far from the middle coat, as if it were inflated, and the space between them contains numerous particles of oil; but, perhaps, this raising up of the outer coat is often produced by water being imbibed while preparing the specimen for examination. Sometimes, but I think only in vessels of less than 1-500th of an inch in diameter, partial enlargements, like aneurismal dilatations or pouches of their walls, are found.

"The vessels most liable to this disease are, I think, the arteries of about 1-300th of an inch in diameter; but it exists generally, at the same time, in the veins of the same or of less size. As a general rule, judging from the specimens hitherto examined, the disease decreases in nearly the same proportion as the size of the vessels, and the smallest capillaries are least, if at all, affected. But there are many exceptions to this rule; and it is not rare to find vessels of from 1-2000th to 1-3000th of an inch in diameter, having parts of their walls nearly covered with the abnormal deposits.

"The principal and first seat of the deposits is, in arteries, in the more or less developed muscular or transversely fibrous coat; in veins it is in the corresponding layer, immediately within their external fibro-cellular nucleated coat; in vessels, whether arteries or veins, whose walls consist of only a simple pellucid membrane bearing nuclei, the substance of this membrane is the first seat of the deposits. In some cases, the outer fibro-cellular coat of both arteries and veins appears to contain abundant fatty matter. But it is seldom that, in an advanced stage of the affection, any of the several coats of a bloodvessel can be assigned as its chief seat; for even in large four-coated arteries they wholly waste, and their remains appear united in a single pellucid layer, of which the whole thickness may be occupied by the deposit."—(Pp. 139–141.)

The seventh, eighth, ninth, tenth, eleventh, twelfth, and thirteenth Lectures present highly instructive views of their respective subjects, which relate chiefly to the origin, progress, and effects of inflammation in different textures. They come, however, too closely upon many of the ordinary subjects discussed in the pages of this Journal, to require to be noticed here unless in the most cursory manner. Recommending these Lectures, therefore, to the special attention of pathological readers, we subjoin the following short summary of the conclusions which Mr Paget thinks to be established, as to the circumstances which may determine the characters of an inflammatory exudation to be adhesive or suppurative.

"To sum up, then, what may be concluded respecting the conditions that, in the first instance, may determine the adhesive or suppurative characters of an inflammatory exudation; they are, 1*st*, The state of the blood,—its diathesis or crasis,—the power of which is evident in that the same material may be exuded in many inflamed parts in the same person; in that this material may exhibit peculiar characters correspondent with those of the blood itself; and in that, in different persons, an inflammation excited in the same tissue, and by the same stimulus, will produce different forms of lymph, corresponding with differences of the blood. 2*d*, The seat of the inflammation, and the tissue or organ affected; of which the influence is shewn by cases in which, with the same condition of blood, different forms of lymph are produced in different parts or organs. 3*d*, The severity, and acute or chronic character, of the inflammatory process, according to which the product deviates more or less from the character of the natural secretion or blastematous effusion in the part.

"The primitive character or tendency of any case of inflammation might be represented as the resultant of three forces issuing from these conditions."—(Pp. 345, 346.)

The products of inflammatory exudation, it is well known, are liable to vary. Sometimes they are cellular, sometimes fibrous, sometimes fibro-cellular, in certain circumstances osseous or at least calcareous. It is true that these varieties may often be traced to the characters of the structure of the tissue originally affected. Yet sometimes this is not possible. The following observations may communicate some idea of the inferences which Mr Paget thinks justifiable on this subject.

"*Fibrous Tissue*, as the result of the development of lymph, is found when the exudation is interstitial in any fibrous tissue; as in ligaments, capsules of joints, and the like. The best examples of it are in the laminated and nodular thickenings of the capsule of the spleen, or the thickening and induration of the periosteum, or the capsule of the hip-joint in chronic rheumatic arthritis. In all these cases the new material is derived from repeated, but not acute, inflammations; therefore, probably, though excessive, it is not widely different from the normal material for nutrition; and, the conditions for nutrition being little disturbed, it is developed into the exact likeness of the original texture with which it is intermingled and confused.

"As the fibro-cellular and fibrous tissues, formed from inflammatory lymph, become more perfectly organized, they are prone to contract; imitating the contraction already described in granulations and scars (p. 236). Hence, in part, the contraction of the wall of the chest after pleurisy, and the various displacements and deformities of organs that have become adherent to adjacent parts; hence, in part also, the contractions of inflamed organs, as of the liver in cirrhosis; hence, too, an addition to the rigidity of joints when the parts around them have been inflamed; and hence, with yet greater mischief, the contractions of the thickened valves and tendinous cords of the heart.

"*Adipose Tissue* may be formed, if not directly from inflammatory lymph, yet in the fibro-cellular tissue of completely organized adhesions. I think it is not often so formed; but, lately, Dr Kirkes found a lung of which all the anterior part was covered with well-organized false membrane; and in part of this was a quantity of perfect adipose tissue, more than four ounces in weight.

"*Elastic Tissue* is sometimes abundantly formed in the adhesions developed from inflammatory lymph. I have not seen it except in such as are completely organized; and I think it is, in this case, as in the formation of scars, a late production. I believe, also, with Virchow,[1] that its formation depends, in some measure, on the membrane that is inflamed; pleural adhesions being most favourable to it. In these it is often abundant; its principal, but always slender, filaments lying in the same general direction as those of the fibro-cellular tissue.

"*Epithelium* I have already mentioned as covering the surfaces of well-formed adhesions. I know of no observations proving whether the epithelial cells are developed directly from the lymph, or are a later construction from materials derived from the blood of the adhesion's vessels; but it is not rare to find, in inflammation of serous membranes, recent lymph-cells presenting many characters indicative of development towards epithelium; flattened and enlarged, and having circular or oval clear nuclei.

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[1] Verhandl. der Phys.-Med. Gesellschaft in Wurzburg, 1850, p. 142. He describes here a peculiar thorny or dentate structure often presented by the elastic filaments in old adhesions.

"*Bone* is often formed from inflammatory lymph. It may appear as a late transformation of lymph that has been organized into perfect fibrous tissue; as in the osseous plates that are sometimes found in the false membranes of the pleura, or in the pericardium. In most of these, however, there is not true bone, but an amorphous deposit of earthy matter, which is imbedded in the fibrous tissue, or which (as Rokitansky holds) is the residue of the degenerated and partially absorbed tissue."—(Pp. 361-363.)

On another question, namely, relating to the formation of bloodvessels in effused lymph, the following conclusions are delivered.

"1. The direct observations supposed to prove that blood is formed in lymph are very liable to fallacy, through the facility with which blood may be accidentally mixed with the lymph, in consequence of haemorrhage during life or after death, or in the preparation of the specimens. Where these sources of fallacy have been avoided, I have never seen anything suggestive of a transformation of lymph into blood.

"2. The development of blood from tissue-cells is limited, naturally, to the earliest period of embryo-life, as if it needed the greatest amount of force for development; afterwards, blood is not formed except through a long process of elaboration, and with the aid of many organs. Its formation, therefore, in the mal-conditions of inflammation is very improbable.

"3. In no specimen of inflammatory lymph have I seen appearances of transitions from lymph-cells to blood-cells, such as we may see in the lymph of the lymphatics, both before and after it is poured into the bloodvessels.

"4. Neither in any lymph have I seen appearances of such stellate cells as the interstitial bloodvessels of the early embryo are formed from; nothing comparable with them has ever come into view.

"5. In the formation of vessels for granulations and the walls of chronic abscesses, all is favourable to the belief that they grow up from the bloodvessels of the adjacent parts; and there are no structures to which the lymph bears so close analogy as it does to these, or to which it is so likely to be conformed in the production of its vessels.

"On the whole, therefore, although direct observations are wanting, I think we may conclude that all the vessels of inflammatory lymph are formed by outgrowth from adjacent vessels, as in the process of repair, and that through these vessels, not by its own development, it derives its supply of blood."—(Pp. 367, 368.)

On the intimate nature, as it has been called, of the process of inflammation, many different conjectures have been offered. The subject is one of extreme difficulty, and probably these difficulties are increased by the constant habit of examining and reasoning upon the minute and microscopical parts of the process. The following passage may be quoted as representing the ideas which Mr Paget concludes to be most natural.

"If we consider the quantity of organic formation effected during the inflammatory process, in the proper substance of the inflamed part, it is evidently less than in health. All the changes described in the last lecture are examples of diminished or suspended nutrition in the tissues of the inflamed part; they are all characteristic of atrophy, degeneration, or death. The tissues become soft, or quite disorganized; they are relaxed and weakened; they degenerate, and remain lowered at once in structure, chemical composition, and functional power; or else, after degeneration, they are absorbed, or are disintegrated, or dissolved, and cast out; they die in particles or in the mass. During all the process of inflammation there is no such thing as an increased formation of the natural structures of the inflamed part; they are not even maintained; their nutrition is always impaired, or quite suspended. It is only after the inflammation has ceased that there is an increased formation in some of the lowly organized tissues, as the bones and cellular tissue.

"So far, then, as the proper substance of the inflamed part is concerned, there appears to be decreased action; that is, decreased formation. There may be, indeed, an increased absorption; but this is also, in one sense, characteristic of decreased exercise of vital force; since all absorption implies a previous degeneration of the part absorbed. Nor can we justly call this, in any sense, 'increased action,' till we can shew how absorption is an action of vessels.

"Thus far, one of the constituents of the inflammatory process, one of the characters in which it differs, in respect of quantity, from normal nutrition, is a defect in the nutrition of the proper substance of the inflamed part.

"But it is characteristic of the complete process of inflammation, that, while the inflamed structure itself suffers deterioration, there is a production of material which may be peculiarly organized. Here, therefore, may be an evidence of increased formation, of increased action.

" Doubtless, in relation to the productive part of the inflammatory process, the expression ' increased action' may be in some sense justly used; for the weight of an inflamed part, or of the material separated from it, may be much increased by the formation of organized matter. But the quantity of organized matter formed in an inflammation must not be unconditionally taken as a measure of increase in the exercise of the vital forces; for it is to be observed, that the material formed presents only the lowest grades of organization, and that it is not capable of development, but rather tends to degeneration, so long as the inflammation lasts.

" It may be but a vague estimate that we can make of the amount of force exercised in any act of formation; yet we may be sure that a comparatively small amount is sufficient for the production of low organisms, such as are the fibrinous and corpuscular lymphs of inflammation. The abundant production of lowly organized structures is one of the features of the life of the lowest creatures, in both the vegetable and animal kingdoms. And, in our own cases, a corresponding abundant production is often noticed in the lowest states of vital force; witness the final inflammations, so frequent in the last stages of granular degeneration of the kidneys, of phthisis, of cancer, and other exhausting diseases. In all these, even large quantities of the lowly organized cells of inflammatory lymph may be formed, when life is at its last ebb. And with these cases those correspond which shew the most rapid increase of tubercle and cancer, and of lowly organized tumours, when the health is most enfeebled, and when the blood and all the natural structures are wasting.

" From these considerations we may conclude that the productive part of the inflammatory process is not declaratory of the exercise of a large amount of formative or organizing force; and this conclusion is confirmed by observing that development, which always requires the highest and most favoured exercise of the powers of organic life, does not occur while inflammation lasts. The general conclusions, therefore, may be, as well from the productive, as from the destructive, effects of the inflammatory process, that it is accomplished with small expenditure of vital force; and that even when large quantities of lymph are lowly organized, such an expression as ' increased action' cannot be rightly used, unless we can be sure that the defect of the formative power, exercised in the proper tissue of the inflamed part, is more than counterbalanced by the excess employed in the production and low organization of lymph.

" It may be said that the signs of inflammation are signs of increased action. But these are fallacious, if again, by increased action be meant any increased exercise of vital force. The redness and the swelling of the inflamed part declare the presence of more blood; but this blood moves slowly; and it is a quick renewal of blood, rather than a large quantity at any time in a part, that is significant of active life. An abundance of blood, with slow movement of it, is not characteristic of activity in a part; it often implies the contrary, as in the erectile tissues, and the cancellous tissue of bones.

"The local increase of heat is too inconstant to afford ground for judging of the nature of inflammation.[1] When manifest, it is not, I think, to be exactly compared with that of an actively growing part, or of one which is the seat of 'determination' of blood, or of 'active conjestion.' In these cases the heat is high, chiefly because the blood, brought quickly from the heart, is quickly renewed; but in an inflamed part the blood is not so renewed; it moves more slowly. The heat may, indeed, be in some measure ascribed to this condition; for the quickly moving blood around the inflamed part may communicate its heat to that which is moving more slowly. But the proper heat of inflammation (I mean that which is measurable by the thermometer) cannot, I think, be wholly thus explained. Some of it is probably due to the oxidation of the degenerating tissues; a process which we might safely assume to be rapidly going on in the more destructive inflammations, and which is, indeed, nearly proved by some of the evidences of the increased excresion of oxidized substances in inflammations, especially by the increase of phosphates in the urine during inflammation of the brain.[2] It is far from proved, indeed, that this source of heat is sufficient for the explanation of the increase in an inflamed part; and it may be at once objected that we have no evidence that the hottest inflamed parts are those in which the most destructive processes are going on. Still, in relation to the question, how far the increased heat is a sign of the quantity of formative force that is being exercised, we may argue that, as the general supply of heat in our bodies is derived from oxidation or combustion of wasted tissues or of surplus food, so, in these local augmentations of heat, the source is rather from some similar destruction of organized substances, than from increased formation of them. If it be so, the increased heat will give no ground for regarding the inflammatory process as the result of a greater exercise of formative force than is employed in ordinary nutrition; none for speaking of it as increased nutrition, or increased action. Rather, this sign may be added to the evidences that the inflammatory process presents, of diminished formative force, and of a premature and rapid degeneration, in the affected part."—(Pp. 430–435.

Mr Paget, in forming an estimate of the influence of different causes in producing inflammation and maintaining that state, assigns its full value to the blood as a primary agent;

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[1] See, especially, v. Barensprung, in Müller's Archiv, 1852, p. 268.

[2] Dr Bence Jones: On the Contrast between Delirium Tremens and Inflammation of the Brain, Med.-Chir. Trans., vol. xxx., p. 37; and Virchow, in his Archiv, B. iv., H. 1.

and adverts to the ideas entertained by many of the great influence of the nervous system, and the nervous supply sent to different tissues. It is quite unnecessary to say one word as to the influence of the first of these causes, further than that, the author gives a good general view of this influence and the circumstances which favour its action, and which prove its reality.

He seems to attach less importance probably to the alleged influence of the nervous system,—an influence always very difficult to demonstrate and to estimate. He allows the ambiguity of the common proofs adduced to shew the influence of the nervous system in the production of inflammation; and this is not wonderful, because many of the instances in which inflammation seems so excited by irritation, may be explained in other modes; and what is called irritation by some, proves to be actual inflammation in its early stage.

For the explanation of the cases referred to, he thinks that there are two hypotheses. 1. It may be that the nerves distributed to the minute bloodvessels of a part, may be so affected that these vessels may be dilated, and their dilatation may produce the other phenomena of inflammation. 2. The disturbance of the nervous force may more directly interfere with the process of nutrition, inasmuch as this force always increases some influence in the nutrition of the part, and is as may be said, one among the plasturgia or tissue-forming forces.

Mr Paget represents the first of these hypotheses as having lately acquired a prominent position in systems of Pathology, especially in Germany; and he mentions as its principal supporters at the present time, Henle and Rokitansky. This is not at all unlikely both as to the nature and as to the individuals, of whose hypothetical opinions we desire to speak with respect. We have merely to remark, that the doctrine of the influence of the nervous system over the bloodvessels, is so far from being new, that scarcely any one has been so much and so repeatedly brought before the profession, by various individuals and in various forms. A list of nearly forty or fifty persons, not to use the language of hyperbole and exaggeration, all of whom have more or less distinctly, and more or less forcibly maintained the influence of the nervous system in inducing and maintaining inflammation, might be mentioned with little difficulty; but we shall merely notice a few.

In the first place, Dr Wilson Philip was a strenuous advocate for the influence of the nervous system in inducing, in maintaining, and in modifying the process of inflammation; and has in various parts of his writings more or less strongly brought forward his doctrines. Secondly, it was a favourite doctrine of the late Mr John Allan, lecturer on Surgery in this city, that the nervous system exerts great influence on the production and the maintenance of inflammation, and that inflammation never takes place without the participation of this system. Not only was this a theory of Mr Allan; it formed the basis of practical measures; he insisted very strongly on the use and advantage of remedies of sedative and narcotic properties in subduing and moderating the process; and above all, the unchemical remedy of acetate of lead and opium.[1]

Mr John Lizars in like manner is a great advocate for the influence of the nervous system in producing and maintaining inflammation; and every one who wishes to see the manner, in which this surgeon maintains the justice of his views, may consult a paper in the fifteenth volume of this Journal, p. 396.

By far the most impartial advocate, however, for the influence of the nervous system in inflammation, is Schroeder Van der Kolk, who undertook to ascertain the value of the hypothesis by performing experiments upon animals, which may be found detailed in the forty-fifth volume of this Journal, pp. 500–506, 1836. Other supporters of this theory, long before the time either of Henle or Rokitansky, might be mentioned.

The question, however, is not whether the doctrine prevails at the present time, and when it originated, but whether it has any foundation in truth. Now upon this point it must be confessed that a considerable amount of plausible reasoning and declamation has been evinced, but little correct induction. Let us not forget to add, that what has been hitherto said, leads to the inference, that the question is not one which admits of decided determination. Mr Paget has so ably condensed the whole of what may be urged in its favour, that it seems necessary here to give his inferences.

> "The eminence of those who have supported this hypothesis makes one hesitate in rejecting it; and yet I cannot help believing it to be groundless. If we remember that parts may present some of the chief phenomena of inflammation, though they have no nerves, as the firmest tendons and articular cartilages; that the degrees of inflammation in parts bear no proportion to the amounts of pain in them when inflamed; that the severest pains may endure for very long periods with only trival, if any, phenomena of inflammation; that the phenomena of the so-called reflex paralysis are rare, equivocal,

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[1] System of Surgery. By John Allan, F.R.S.E., &c.

and altogether insufficient for the foundation of a law or general principle; we may well think that there can be no sufficient ground for the invention of such an hypothesis as this. And, if we add that, even admitting the dilatation of bloodvessels as a possible consequence of the stimulus of sensitive nerves, yet the phenomena of even simple inflammation would be no necessary consequences thereof; that the varieties of inflammations would be quite unintelligible as results of similar mechanical disturbances of the circulation; and that the dilatation of bloodvessels, in any mechanical way produced, is followed by only feeble imitations of a part of the inflammatory process; then we may think that the hypothesis, if all its postulates be granted, will yet be insufficient for the explanation of the facts.

"I believe that, if we would have any clear thoughts respecting the influence of the nerves in initiating inflammations, we must first receive the theory already referred to, that a certain exercise of the nervous force is habitually and directly engaged in the act of normal nutrition. If we admit this, there can be little difficulty in believing, whatever there may be in explaining, that the perturbations of the nervous force may engender the inflammatory mode of nutrition more directly, than by first paralysing the bloodvessels of a part. We attain nearly to a proof of this in the instances of altered nutrition adduced in a former lecture, and in those of secretions altered, not in quantity alone, but in quality, by affections of the nervous system. It is almost inconceivable that any of the essential properties of a secretion should be changed by an alteration in the quantity or movement of the blood in a gland; yet such changes are frequently manifest in the milk, tears, urine, and sweat, under the influence of mental affections of the nervous force; and the analogies of secretion and nutrition give these cases nearly the weight of proof, in the question of the influence of the disturbed nervous force in causing inflammations."—(Pp. 447, 448.)

It is unquestionably true, that the experiments of M. Claude Bernard, Professor Budge, and Dr Waller, which are given in a former part of this number, tend to shew, that the state of the nerves has very great influence over the capillary vessels; that their injury is followed by a serious lesion in the circulation of the parts to which the vessels are sent; and that, as formerly taught by Dr Wilson Philip, the effects of this lesion are temporarily suspended by the use of the galvanic stimulus. But these experiments do not hitherto, in the opinion of impartial judges, establish the inference, that inflammation is in this manner induced. They merely shew that the energy and the regularity of the circulation is impaired. In short, the whole question seems to be one, upon which at present it is impossible to adduce satisfactory evidence, or anything more than a certain degree of probability.

We are warned, however, that it is time to close these observations. In so doing, it is only an act of justice to say, that the Lectures of Mr Paget contain a large amount of valuable information, and that they deserve to be studied with the utmost attention, by all who wish to obtain just ideas on the intimate nature of the morbid changes to which the human body is liable. We began by saying, that we doubt the propriety of the distinction of Pathology into Medical and Surgical. If anything could directly shew that the distinction is arbitrary and superficial, it is the present work and the information which it contains. It may be called a system of Surgical Pathology in so far as it treats of diseases, many of which require the aid and interposition of the surgeon. But it is a work on Pathology generally; and cannot fail to be at once necessary and serviceable to the physician as well as to the surgeon.

ART. II. — *Change of Climate considered as a Remedy in Dyspeptic, Pulmonary, and other Chronic Affections. With an Account of the most eligible Places of Residence for Invalids, in* SPAIN, PORTUGAL, ALGERIA, *&c., at different Seasons of the Year; and an Appendix on the Mineral Springs of the Pyrenees, Vichy, and Aix les Bains.* By D. J. T. FRANCIS, M.D., London; Corresponding Member of the Society of the Medical Sciences of Lisbon, of the Academy of Medicine and Surgery of Barcelona, &c.; Physician to the Dispensary for Consumption and Diseases of the Chest, Margaret Street, Cavendish Square. London, 1853. 12mo. Pp. 339.

In the seventy-fourth volume of this Journal, for July 1850, was given an account of the Treatise upon the Climate of Madeira, by the late Dr J. A. Mason, especially considered as a residence for invalids threatened with, or labouring under, symptoms of Pulmonary Consumption. The evidence was rather against the island of Madeira as an eligible situation; and we felt it to be our duty to direct attention to the fact, that there are on the habitable globe, at least from forty to sixty places all nearly as accessible from England as Madeira, and the whole of them possessing quite the same appearances of tropical and engytropical regions. Among these places we mentioned the south of Spain, and, since the French steam-vessels sail so regularly between Marseilles and Algiers, the northern coast of Africa. Our purpose was to shew, that places on the south coast of Spain and the northern coast of Africa were rather, upon the whole, better suited to form residences for bronchitic, pulmonary, and phthisical invalids, than either Madeira or any of the Atlantic islands. At the same time we felt it necessary to add, that, though these situations possessed several of the requisites supposed to be most beneficial to pulmonary invalids, namely tropical and engytropical temperature, mildness of climate, and almost no winter such as we witness and suffer in our northern latitudes, yet it must not be inferred, that the laws by which the physical world is regulated, are there suspended, and that the usual characters of the seasons, and the extremes between the high temperature of the day and the low temperature preceding early dawn are to be effaced.[1]

It has always been an opinion with us that the Peninsula, especially its southern coast, has been too little tried as a residence for invalids. Situate between the thirty-sixth and thirty-eighth degrees of north latitude, it stretches further south than the south of Italy; and the southern half at least of this extensive region presents many places, the temperature of which can never be very low, and the climate of which is believed to be mild and genial for a longer period of the year than that of any European country. From its being surrounded, also, on three sides, and the greater part of its fourth side by the sea, its temperature ought to be more equable and uniform than countries more completely inland. Notwithstanding these apparent advantages, however, since the time at which Dr Withering repaired to Lisbon in 1792, to try the effects of the Peninsular climate in alleviating the symptoms and retarding the progress of serious disease of the bronchial membrane and lungs, and described with fidelity the effects of a residence there, little has been done by physicians to explain and recommend either the coast or the interior of Spain or Portugal as a residence for persons who are threatened with, or are actually labouring under, the symptoms of pulmonary disease.

This duty, however, Dr Francis has undertaken upon a large scale, and in a systematic manner. The present work is devoted principally to the examination of the climate of the Peninsula considered as an agent, prophylactic and therapeutic; but the author also gives an account of the climate of Algiers, and subjoins in an appendix some useful information on the mineral springs of the Pyrenees and Auvergne.

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[1] Edinburgh Medical and Surgical Journal, volume seventy-fourth, pp. 177, 178.

The work consists of two parts; the first on climates in general, and the effects of climate as a means of curing various diseases. The second part is more expressly devoted to the examination of the various situations in Spain and Algiers, which may be regarded as most eligible for the abode of invalids.

Dr Francis justly remarks, that the climate of Spain is little known in England and to inhabitants of Great Britain, though it is superior, in his opinion, to that of Italy. He directs attention to the fact, that, in the latter country the long chain of the Apennines traversing the country from north-west to south-east, affords no protection from the quarters most trying to the human frame; while their cold and often snow-covered ridges approach so near to the sea in many points of their course, that the warm and low-lying districts experience all the evils of frequent and sudden oscillations between the sea and land winds. The whole of the Mediterranean coast of Spain, on the other hand, may be said to have a southern aspect; it has also a larger extent in the direction east and west, while the mountain chains lying mostly east and west, contribute more decidedly to protect the southern part of the country from the chilling winds of the north and north-east. The principal disadvantage under which the south coast of Spain laboured, that of distance and inaccessibility, has been greatly diminished by the use of steam-vessels, by means of which the most southern points of the Peninsula are brought within the compass of a six, seven, or nine days' voyage.

In his general observations on the influence of climate, and different kinds of climate, Dr Francis adduces all the usual facts, by means of which a judgment may be formed, as to the influence, which any given climate may exercise upon the health of natives and visitors. These it is unnecessary here to reproduce, as we have so often in this Journal, especially in considering the work of Sir James Clark, and the recent treatise by the late Dr Mason, had occasion to enter at sufficient length, and with a good deal of detail, into these circumstances. The general result, which is the principal point of practical importance, is, that the climate of Spain possesses advantages over that of Italy, and very considerable advantages over the south of France, and indeed that of any country in Europe. Though warm, and with a constantly rather high temperature, the mean annual temperature is from 57° to 58°, yet it is not a relaxing climate.

Dr Francis maintains also that the climate of Spain is not what may be called an irritant climate; that is, it does not excite the nervous and the vascular systems. It appears that the author means the irritation of the bronchial and digestive mucous surfaces. For the representation, without this construction, is at variance with the testimony of all writers and travellers who have visited Spain; according to whom there are periods during the summer and autumnal season, when this irritant power evinces its action by the private quarrels and sanguinary acts of the natives. The influence of solar heat, and the irritant nature of the climate, is so well understood, and so generally admitted, that the judges, in deciding upon the degree of guilt of various criminal acts, always make allowance for the influence of physical and climatic agency.

The diseases in the treatment of which Dr Francis considers change of climate to be beneficial, are, in the first place, pulmonary consumption, chronic bronchitis, with or without emphysema of the lungs, chronic affections of the larynx and windpipe, and the obscure and imperfectly defined disorder, which passes under the name of spasmodic asthma; indigestion in its atonic form, in its nervous form; but the inflammatory only after symptoms of irritation have been mitigated by proper treatment. But in the management of these forms of disorder Dr Francis warns the reader that he must not choose his climate at random; and he explains the principles upon which physician and patient should select the kind of climate best accommodated to each form and stage of the disorder.

The author next considers how far change of climate is a proper remedy for the removal of various nervous disorders; for the relief of dropsy, and urinary, that is, calcutous and renal diseases; Rheumatism, Gout, Strumous diseases; for Anaemia, Cachexia; Uterine torpor; for delicacy of constitution; and for the effects of general mental exhaustion.

In the Fifth chapter, the last of the first division of the work, the author gives for the guidance of invalid travellers, a series of directions which are judicious and promise to be serviceable.

In the second part, Dr Francis enters on the particular subject of his work; and takes a view of the physical characters of the climates of Madrid, Aranjuez, La Granja; Lisbon, Cintra, Seville, Cadiz, and the adjoining towns of Chiclana, Medina Sidonia, and Gibraltar; and communicates on the health of the inhabitants, the most prevalent diseases and similar points, a good deal of information, which, though not absolutely new, is unquestionably interesting, and given in an agreeable manner. All these places are more or less suitable for the residence of invalids, especially those with bronchitic and pulmonary symptoms; and the author gives not a few decisive proofs of the mildness of the climate, and the beneficial influence which it exerts on the health of the human race, from the kind of the vegetable products, and the healthiness and longevity of the inhabitants. Not one of these places, however, according to Dr Francis, comes near to Malaga in this point of view; and this city and its vicinity he represents as blessed with by far the finest climate in Europe. As it is important that the advantages of Malaga as a residence for invalids should be extensively known, we subjoin the description as given by Dr Francis.

It is scarcely necessary to say that Malaga is a town situate on the south Mediterranean coast of Spain, in a nook or recess at the depth of a bay, trending from Gibraltar to the north-east, in north latitude 36° 43′, and west longitude from Greenwich 4° and about 20′.

" There is no place in Spain, nor in the whole of Europe, as far as our present information goes, that possesses a climate at once so mild and equable, with so little variation from day to day, and from day to night, as Malaga. Situated in the 36° 43′ of latitude, it is far to the south of any portion of the Italian Peninsula, and even of Sicily and Greece. In addition to this advantage it will be found to possess, to a great extent, those favourable physical conditions of the surrounding country which give to a southern latitude its proper value.

" Winter indeed can hardly be said to exist; a perpetual spring, during which vegetation proceeds unchecked, connecting the autumn of one year with the summer of the next. The natives, fully alive to the delicious character of their climate, spend a large portion of their lives, and seek their amusements in the open air; whilst many of the poor, the whole year through, care for no other bed than such as they can spread after nightfall upon the public walks. Spanish writers vie with each other in describing the praises of Malaga, which seems to them a species of paradise even in Andalucia.

' Aqui no llega el frio ni la nieve,
Y corren apacibles de continuo
Los maritimos zéfiros suaves,
A recrear los hombres dulcemente.'

" As this is the winter residence which seems to me pre-eminently, among the chosen places of Europe, to promise to the invalid who is threatened with, or already the subject of, pulmonary consumption, in its earlier stage, such relief as change of climate can accomplish for him, I purpose to enter as fully into a consideration of the subject, as the materials at command at the time of my visits permit me. These visits, three in number, were made during the winter and spring; and the information about to be detailed was derived from personal observation, from visits to the hospital, and from conversation with the natives, and with invalids who had passed some time at Malaga for the benefit of their health. My acknowledgments are especially due to the British consul, Mr Mark, whose kind and zealous assistance placed at my disposal a valuable report that had been drawn up by his late father, and procured for me, from the authorities of the city, official documents upon the mortality, &c.

" Like many other Spanish towns on the Mediterranean, Malaga is built upon one of those fruitful, plainlike tracts of ground which occur at intervals along the shore, having the sea on the south side, and a more or less complete semicircle of mountains in the opposite direction. To the north and west the view is bounded by an almost continuous chain of hills, about three thousand feet high, which rise at a distance of from one to two leagues from the town, and are formed up to their summits into vine terraces. Behind these, and separated from them by deep valleys, is a still higher Sierra. This arrangement seems to act as an effectual barrier to the cold winds which during the winter come down from the summits of the loftier mountains, and which are either arrested or modified by the secondary chain under the protection of which the town lies.

" A lower range of undulating vine-covered hills branches off from the first-mentioned chain, and running down to the water, approaches close to the town on the east side. A tolerable, though not a complete screen, is thus afforded in that direction.

" To the south the houses slope down to the water's edge; and the mountains are sufficiently far removed in the west to permit the full influence of the afternoon sun.

" The older portion of the town,—a labyrinth of very narrow and winding lanes and tall houses, where the duties of the scavenger are imperfectly performed,—gradually ascends, after the Moorish manner, upon rocky ground, towards the Alcazar or castle. The newer and level portions, however, are built upon a sandy soil that has been reclaimed from the sea, which is rapidly retreating from the shore.

" Through the centre of this latter quarter runs the Alameda, a noble promenade between forty and fifty yards wide, planted with acacias, and adorned with statues and fountains. On either side of the walk are houses conspicuous for their cheerful appearance and elegance; and these, as well as others in the short avenues running from the Alameda to the sea, having been recently built, have had proper attention paid to convenience and ventilation. Indeed, the whole ventilation of this immediate district is admirably provided for by the unusual breadth of the streets, and by their proximity to the sea; so that, as the mid-day breeze springs up, gentle streams of air play freely among the houses.

"In this part of the town the invalid should take up his residence, preferring those houses which have a southerly aspect. There is here a cheerful sunny walk, upon which he may at once step out from his door as if it were his own garden. The next best situation is to be found upon the quay; which would be even preferred, probably, by those who take an interest in the bustle and movement of a port.

"The little plain, or Vega as it is locally called,—at the south-east extremity of which Malaga lies, is a succession of gentle undulations, gradually rising towards the mountains, and forms one continued garden of oranges and lemons, palms, corn, almonds, figs, olives, and other choice productions of these climates. Its soil is partly alluvial, and partly sandy; there are no standing waters; and it is worthy of remark that, unlike many similarly situated tracts of land in Spain, there is here no general system of irrigation, which, however favourable to vegetable production, is decidedly objectionable in a sanitary point of view. Such irrigation as there is, is confined to the orange and fruit grounds; and the water being raised for this purpose by the toil of the ox or the mule at the Noria, is necessarily limited in supply. The corn crops, indeed, are, as I was assured, never known to fail; and being indebted entirely to nature for the amount of moisture which they receive, furnish a proof, that the neighbourhood of Malaga is less dry than portions of the provinces of Murcia and Alicante, which border upon the Mediterranean, where the rich soil, unless artificially watered, is practically barren.

"A considerable river, the Guadalhorce, enters the plain at its north-west extremity, from among the mountains, and falls into the sea about three miles to the west of Malaga. In addition to this, there is a small stream, which, washing one end of the town, separates it from one of the suburbs. This stream, which, generally speaking, is little else than a rapid brook flowing in the centre of a wide bed, performs to some extent the part of a sewer.

"From what has now been said, one might justly predict that the situation of Malaga would be favourable to the production of that mild and healthy climate, which is, in reality, found to exist there. The great attractions of its landscapes, moreover,—a well-broken shore, numerous ranges of mountains, reaching from the lowly vine slope to the stupendous sierras of Granada, a wide expanse of sea, bounded by the distant outline of the Barbary mountains, and the sunny plain itself, abounding in pleasant groves and gardens, provide another element in what is required by foreign visitors of such places. The walks and drives, also, are both numerous and varied; and it will be observed of Malaga, as of most other Spanish towns, that you can ramble forth into the country without being hemmed in by those long lanes of high stone walls which so hopelessly obstruct the view in the nighbourhood of many of the towns of Provence and Italy.

" The climate of Malaga, if we except its temperature, is chiefly remarkable for the absence of any strongly marked or positive qualities, and may be said to hold an intermediate position, in respect to its effects upon the constitution, between the oppressive and subduing influence of Madeira and Rome, (and) the stimulating, bracing character of Nice. It is moist, compared with the latter; dry, compared with the former. Its greater proximity to the Gibraltar Straits renders it also less dry than several other parts of the Spanish Mediterranean coast. At the same time, the character of the air inclines rather to dryness than to moisture.

" *Temperature.*—The mean annual temperature of Malaga is 66·11°, being warmer than Madeira by two degrees, than Rome and Pisa by six, than Nice by seven, than Pau by ten, than London by sixteen.

" On the other hand, it is colder than Malta by one, and Cairo by six degrees.

" Compared with other places in the Peninsula, it is warmer than Lisbon by five, and than Cadiz and Valencia by three degrees.

" This comparative result as to warmth is due, in some measure, to the intensity of the summer heat, but chiefly to the mildness of the winter.

" Thus the mean winter temperature of Malaga is 54·41°, being six degrees warmer than Rome, seven than Nice, eight than Pisa, thirteen than Pau, fifteen than London. It is, however, six degrees colder than Madeira, four than Cairo, three than Malta.

" It may give a clearer idea of the mildness of the winter of Malaga, if we bear in mind, that, in regard to temperature, the month of January, in that place, corresponds with May in London, with June in Edinburgh, with April in Pisa and Rome.

" The mean temperature of spring is 62·55°, being identical with Madeira and Malta, but five degrees warmer than Rome and Pisa, and eight than Pau. The summer and autumn temperatures are 78·38° and 68·67° respectively.

" The mean annual range of temperature is 49°, being many degrees less than that of any other place on the continent of which we possess records; the range at Pau being 68°, at Rome 62°, at Nice 60°. It is surpassed, however, in this respect by Madeira, the range of which is only 31°.

" The mean daily range, which is a point of far greater importance to the invalid, inasmuch as it is that by which chiefly the essential feature of unchangeableness is to be tested, in Malaga amounts to

4·1° only. In this particular, we find the climate superior to any of those that have been hitherto noticed; the daily range of Madeira being 9½°, of Rome 10°, of Nice 9°. In connection with this circumstance, it may be observed, that it is far easier for a person in delicate health to guard against variations of temperature from day to day, where they exist, than from similar variations at different periods of the same day.

"The mean difference between the temperature of successive months is 4·95° for the whole year. For the winter months only, in which we are chiefly concerned, it is 2·16°.

"I have already referred to the constancy of the temperature of Malaga throughout the twenty-four hours; but, perhaps, its little changeableness from day to day is even more remarkable. On inspection of the tables from which my conclusions have been drawn, the recurrence of the same degree of warmth, and of the same winds, in the morning, noon, and evening observations of several consecutive days, happens so frequently, that the impression of some error having crept into them, at once suggests itself. I was assured, however, on mentioning the circumstance to a gentleman, of whom I shall make further mention, who had himself carefully registered the temperature during six contemporaneous months, that his own figures shewed the same peculiarity, and fully corroborated, in other respects, the correctness of the data which I have employed. Mr Mark, the consul, also, among others, told me that his own observations lead to the same result.

"Although abundant corn and other crops are produced in the plain of Malaga without artificial irrigation, rain is but of rare occurrence, falling approximatively (for an exact account does not exist) on forty days in the year. Observations made at early morning, noon, and evening, during three years, shew that, on an average, rain fell on sixteen days in the year, at one or other of those periods. On one day in each week, however, no notes were made. Extending, therefore, the calculation in the like ratio to the remaining fifty-two days, the number would be increased to nineteen. An independent register, kept by an English visitor, for the year ending March 1848, the results of which were kindly communicated to me, differs but little in the number of rainy days, viz. sixteen, from that which I have given.

"Neither of these statements, however, includes the rain that fell during the night, for which more than sufficient allowance is made, in assuming the whole number of rainy days in the year to be forty.

"In general the month of May has the largest share of these. The remainder are divided nearly equally among the winter and autumn months, whilst July and August are absolutely rainless.

"Still, owing to other causes, the air is so far charged with moisture, that in those times of the year, when the evening temperature is more than usually cooler than the day, a copious fog is seen to cover the plain as the sun leaves the horizon.

" The Malaganian who passes his life in his own town has no practical acquaintance with ice beyond such as he may make at the confectioners. The consul, speaking from an experience of twenty years, told me that during that time there had been but one transient visitation of ice, so slight, indeed, that it was barely perceptible.

" In regard to the barometer, the same indisposition to change is observable as in the case of the temperature. Perhaps the most practical inference which an inspection of the tables discloses is the fact, that as many as twenty consecutive days not unfrequently happen, during which the amount of atmospheric pressure, morning, noon, and evening, is identical. Its mean point lies between twenty-seven and twenty-eight.

" I have before mentioned that the protecting chain of mountains around Malaga is incomplete at one point, to the north-west, where the river Guadalhorce enters the plain at a distance of three leagues from the town. From this quarter the wind, known locally as the terral, does occasionally blow. It is dry and stimulating, apt to be accompanied with dust, and depresses the sensible temperature. In ordinary winters, however, its occurrence is comparatively rare.

" Here the remark, which I have before made, may be applied, that no climate is absolutely good or perfect for the purposes of the invalid. It is good only in comparison with others that are inferior, such, for instance, as the climate that may have been left behind.[1]

" When such winds blow, here as elsewhere, we must be content to frequent those spots only that are well protected from their influence ; and it will be useful to remember how trifling is the restraint thus put upon us, compared with what it would have been through the cold, windy, rainy days of a long winter in England.''

Dr Francis then adduces various facts to shew that Malaga was a healthy place in 1829, and is so in 1852.

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[1] " The prospect of discovering a climate that will fulfil all that can be desired is almost hopeless. Still it may be asserted that Malaga will not afford grounds for disappointment, similar to those referred to in the following account, which was given me at Cadiz in January 1849 by Commodore B., at that time commanding the American squadron in the Mediterranean. He had just arrived from Madeira, where he had passed not only the last three, but considerable portions of the last fifteen months, for the relief of slow consumption. He said that for three months past, the weather at Funchal had been more frequently rainy and chilly than fine, besides being very changeable; and for several weeks the high mountains near Funchal had been covered with snow, the cold air descending from which influenced very considerably the temperature of the town. In summer the heat was unbearable in the low country.

" This gentleman spoke also of the absence of level walking ground, and of the general dullness of the place. He found the climate of Cadiz more agreeable than the one which he had left."

[This Note may be used as a comment upon the representations made by Dr Lund.]

"The statistical information which I am able to adduce in support of these statements is contained in the following tables, which I have condensed from more detailed particulars. The results of the first two years were published in the 'Avisador Malagueño;' the last were obtained for me through the kindness of the present consul from official sources.

AGE.	1846.	1847.[1]	1848.
Below 1 year	549	...	444
From 1 to 5	312	...	492
„ 5 — 10	29	...	88
„ 10 — 15	28	...	24
„ 15 — 20	59	...	60
„ 20 — 25	101	...	128
„ 25 — 30	81	...	92
„ 30 — 40	156	...	148
„ 40 — 50	154	...	156
„ 50 — 60	140	...	204
„ 60 — 70	134	...	224
„ 70 — 80	118	...	120
„ 80 — 90	61	...	84
„ 90 — 100	25	...	20
100 and upwards	10	...	...
Total Number of Deaths.	1957	2643	2284

"The population of Malaga during the last few years has increased very rapidly, and is now generally considered to reach at least 85,000. The ratio of mortality deduced from the above table will be, consequently, in whole numbers, 1 in 37. The average duration of life is approximately 28 years, as shewn by the same table.

"Favourable, on the whole, as these results are, it will be necessary, before a comparison can be fairly instituted between Malaga and other places in regard to mortality, to take into consideration one obvious source of fallacy, viz., the unusually large proportion of deaths taking place in infancy and early childhood. This is a common result wherever the system of the abandonment of infants (niños expositos) by their mothers is practised to so sad an extent as in many parts of the Peninsula.

"Thus we find that in Malaga 23·4 per cent. of the population die during the first year of life, and 42·3 per cent. during the first five years.

"Another circumstance to be borne in mind, in regard to the mortality of early manhood in Malaga, is the existence of a large convict establishment, and of a large garrison in the town; including, together, seldom less than 6000 adults; generally a much larger number.

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[1] "The classification this year, 1847, is based on no uniform plan, and in one quarter is deficient.

" The tendency of the population is to increase rapidly. In the years 1846 and 1847 there were 6309 births, and 4600 deaths; shewing an excess of births over deaths of 1709 in two years.

" Without pushing the inquiry any further, and taking the evidence derived from it for no more than it is worth, the salubrity of Malaga is placed in a favourable position.

" But the tables disclose, further, a fact of greater interest—the extraordinary longevity of the people. Mr Mark in his MS. report says, referring to this subject, 'Ten died last year (1828) (in preceding years from 6 to 8) of 100, 102, and 105 years, and many others from 95 to 100. Persons of from 80 to 90 years of age may be seen going about the streets with the full use of all their faculties.' I have myself witnessed, in the Casa de Misericordia, as well as in the town, many similar instances; and all that I learned from various persons in Malaga went to shew, that the opinion commonly held there upon the subject is in accordance with the result which an inspection of the table discloses.[1]

" It remains to consider briefly the practical application of the foregoing remarks. Calm, mild, and equable, free from the extremes of dryness or moisture, the climate of Malaga is well fitted for promoting a steady and uniform action of the diferent functions of the body. It affords at the same time the opportunity of escaping, in great measure, from those many causes of fresh disease and of aggravations and relapses into old ones, which are almost inseparable from cold or unsettled climates.

" The chief advantages, therefore, which Malaga offers us, will be in the treatment of diseases of the organs of respiration. And theory will only suggest what actual observation has to some extent demonstrated, that the negative or *medium* qualities of the climate enable it to embrace within the sphere wherein it promises to be useful, various modifications, and even opposite conditions of the same disease. There are very many cases, for instance, where the stimulating air of Nice, or the subduing influence of Pisa or Madeira, are found to be hurtful, that would derive much more benefit from the climate of Malaga.

" Pulmonary consumption, in its period of threatening merely, or occasionally in that limited subsequent stage, when alone it is prudent for the invalid to remove from home, is the disease for which Malaga is particularly suited. If the climate of southern Spain is destined ever to find favour with many of the invalids who leave our shores so affected, it will be to Malaga that attention will be directed. Mr Mark, whose experience extends over several years, tells me that many persons from England, affected in one or other of the above-mentioned ways, who have at different times spent a winter in Malaga, have almost uniformly experienced considerable benefit. And it is not unusual to find among such persons several who, after having visited the more highly lauded places of resort in other countries, are able to express their preference for Malaga.

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[1] " On the 26th December (a day selected at random), there died in Malaga eleven males, of the ages 105 years, 80, 60, 47, 26, two of 22, one age unknown, and three infants; and four females, whose ages were 98, 86, 80, and 73. Eleven persons with a collective age of 699 years. See official return in 'La Cronica.' "

" One remarkable such instance I found in a gentleman of delicate general health and phthisical conformation, who had quitted England on account of haemoptysis. He had spent one winter in Madeira, but spoke in strong and grateful terms of the more decided comfort and health which he experienced at Malaga. In another such case I heard of a similar preference in comparison with Palermo.

" Of various other ailments, such as bronchitis, asthma, laryngitis, rheumatism, dyspepsia, scrofula, and general delicacy of constitution, it will be unnecessary to do more than point them out as likely to be greatly benefited by the air of Malaga. Of chronic laryngitis, indeed, and chronic irritation of the throat, accompanied with more or less complete loss of voice, I have myself known cases of marked relief or cure, and I have heard of similar favourable results that have followed a residence at Malaga in other instances of the same affections.

" It will of course be borne in mind, in the recommendation of a change of climate in the above diseases, that, in those exceptional cases where the disease possesses a very decidedly unmixed and pronounced character, such, for instance, as a pure atonic dyspepsia, or simply relaxed largely secreting bronchitis, more advantage would probably be found in a drier and more stimulating climate. In such cases, Montpellier, and especially Nice and the sea-coast of Provence from Cannes to Toulon, would be more useful.

" Evidence has now been produced,—in the mildness and constancy of the winter climate, in the state of the vegetable kingdom, in the longevity and health of the inhabitants, and, to a certain, although as yet a limited extent, in the beneficial results experienced by invalids who have resided there, of a nature highly favourable to Malaga. Further inquiry will, there is every reason to believe, still more exalt its merits in comparison with many other localities."

We have not very strong or decided opinions upon the curative effects of warm climates in relieving the symptoms of pulmonary consumption; and it is our wish that readers, whether of the profession or not, should form their own conclusions, and estimate the weight of evidence in their own way. But we allow that the facts and considerations adduc by Dr Francis, possess so much force, that we think his propositions are justly entitled to a fair trial. We have already upon various occasions said so much upon the supposed sanability of consumptive and other pulmonary diseases by change of climate, and particularly by removal to a warm and temperate climate, that it is scarcely possible to add to what has been said, with the hope of placing the question in a more clear light. There are, unfortunately, cases of pulmonary disease, to which no climate, no change of climate, can afford any relief, and in which the idea of either cure or recovery is quite chimerical. It would exceed the limits within which these remarks must be compressed, to enumerate and explain the circumstances of these cases; nor is it always possible on this matter to lay down general rules; but where all other situations have been found so useless, and often so injurious, it may certainly be reasonable to try this highly favoured and not less highly eulogized climate of Malaga. Meanwhile, before a long time elapses, we have no doubt that a considerable amount of evidence on the sanative powers of the climate of Spain will be adduced. It is to be hoped that when this has been done, it will be productive of a smaller degree of disappointment than has hitherto been caused by the disproportionate results obtained from residence in other situations, to the expectations created by the characters conferred upon these places by those by whom they were recommended.

ART. III.—*Sandgate as a Residence for Invalids.* By GEO. MOSELEY, Member of the Royal College of Surgeons of England; Licentiate of the Society of Apothecaries; Fellow of the Medical Society of London; Member of the Pathological Society of London, of the Epidemiological Society of London; and Surgeon to the Sandgate Dispensary. London; Sandgate, 1853. 12mo. Pp. 136.

On the coast of the English Channel between the North Foreland and Beachyhead,—we may say as far as the Bill of Portland,—is a long series of seaport towns, havens, and sea-coast villages, all partaking in the common character of being pleasantly situate more or less closely on the beach. One of these is Sandgate, about two miles to the east of Folkstone. Fifty years ago, in the beginning of the present century, Sandgate existed only as a sea-coast hamlet, or cluster of a few houses upon the coast; now it is a large and rather populous town, with numerous new houses well built and pleasantly situated. Shops,—provided with all the luxuries of London, according to Mr Moseley,—are numerous. The means of luxury and enjoyment, he adds, are cheap and abundant.

Sandgate is distant sixty-eight miles south-east from London, and eight miles west from Dover. It is easily accessible from the Folkstone terminus of the South-Eastern Railway; and conveyances of different kinds may be easily obtained to convey visitors at moderate rates by the road, which passes under the cliffs to the town.

The author represents the mean temperature, as deduced from a total period of nine years, to be 62·14°. This is simply a most extraordinary mistake. The numbers which he gives are for ten years, from 1842 to 1851 inclusive; and the mean temperature is not 62·14°, but 52·14°. In short, it can by no possibility be otherwise; for the temperature for four years is 51° and a fraction; for five years, 52° and a fraction; and for one year, namely 1846, it is stated at 54°; so that by no means of calculation could the temperature be made to rise to 62·14°, which, further, is the mean temperature of Cadiz and the south of Spain. The author at a subsequent page (31) states the mean temperature of the seasons of winter, spring, summer, and autumn, to be respectively, 41·76°, 49·50°, 61·30°, and 56°.

He gives a number of tables indicating barometric averages, and the amount of rainy days in different months, which, if correct, may be useful in enabling physicians to judge of the sanitary qualities of Sandgate.

Judging from a Table of Diseases kept at the Sandgate Dispensary, the most prevalent complaints among 1000 cases treated between the 24th June 1844 and the 30th June 1847 are;—ContinuedFever (120), Intermittent Fever (102), Rheumatism (39), Inflammation of the Lungs (38), Diseases of the Skin (38), Indigestion (35), Diarrhoea (31), Scrofula (30), Consumption (26), Hooping-Cough (23), Ophthalmia (21), Scarlet Fever (18), Catarrh (17), Atrophyand Marasmus (15), Measles (15), Neuralgia (13), Worms (9), in the order mentioned. The others are too few or too little connected with the physical influences of the place, to justify any positive inference.

The population of Sandgate is stated at 1400 persons. But it appears that patients come from Hythe, Cheriton, Winchelsea, Romney, and all the neighbouring towns and villages. The population of Hythe is stated at 2675; that of Cheriton at 1658; and that of Romney at 1053. Hence it is requisite to explain, that of the 102 cases of intermittent fever, only ten were from Sandgate; and at the time specified, between 24th June 1844 and 30th June 1847, the population was not more than 1000 souls. The greater part of these ague cases come from Romney and the low grounds in its neighbourhood.

Mr Moseley adduces, as a great proof of the salubrity of Sandgate, the fact that in 1849, when cholera visited different parts of the country with considerable severity, the distemper never appeared at Sandgate.

It would be wrong to doubt that, like other maritime towns, it has the advantage of fresh sea-air and good ventilation, and may therefore be an excellent place for the inhabitants of London and other crowded inland towns to repair to during the summer and autumn. It appears to be chiefly in indigestion and nervous complaints that the air of Sandgate is beneficial.

To all those who wish to learn wherein its advantages consist, the volume of Mr Moseley may be justly recommended.

Death of Mr Bransby Blake Cooper, F.R.S.

The medical profession and the public generally will hear with regret that this most estimable surgeon expired very suddenly on the afternoon of Thursday, the 18th of August, 1853, at the Athenæum Club.

Mr Cooper, who had been in imperfect health for some time, was crossing the lobby of the Athenæum Club Room, when he felt seized with a sensation of faintness. He called for a glass of water; but before it was received, he fell down, with blood issuing from his mouth, and in a few minutes was lifeless.

The body was inspected anatomically in the presence of Messrs J. Birkett and Alfred Poland on the 20th of August 1853, at eight in the morning, about forty-one hours after death, and the following facts were ascertained.

At the root of the tongue, and mostly on its right side, was a deep excavation, covered over by the cortex of the organ, which seemed to form flaps to the hollow. Around this excavation, the tissues of the tongue itself, as well as the muscles about the os hyoides, were infiltrated with the elements of carcinoma.

Several enlarged lymphatic glands existed upon the right side of the neck, all infiltrated with carcinoma. From one large gland, and from another smaller, when cut, a yellow, tenacious, muco-purulent fluid came away. The parietes inclosing this fluid was carcinoma.

The trachea and bronchi, as well as the air cells of the lungs, contained fluid blood, and patches of cells infiltrated with blood appeared upon the surfaces of both the lungs. The tissue of the lungs was healthy, and not a single pleural adhesion existed on either side.

The heart was small, and its muscular tissue thin and pale. The valves were healthy.

The stomach contained imperfectly masticated food, and much blood-coloured fluid.

The tissues of the alimentary canal were healthy.

The liver was rather small, soft, and congested with blood.

The pancreas was not diseased.

The spleen was extremely contracted and corrugated, and of a dark, almost black hue.

Both kidneys were congested with blood, soft, and easily separable from their proper tunics.

In the cavity which existed in the tongue there were a few clots of blood, and as this was the only ulcerated surface, it may be presumed that the hemorrhage proceeded from a perforation in the right lingual artery, or of one of its branches.

Bransby Blake Cooper was the fourth son of the Rev. Samuel Cooper, the elder brother of Sir Astley Cooper, and grandson of Dr Cooper, for many years vicar of Great Yarmouth, in which town the subject of this short notice was born on the 2d September 1792. After receiving in the same town the elements of general education, he at an early age entered the naval service of his country as a midshipman in the Stately, a 64-gun ship, under the special care and instruction of the first lieutenant, afterwards Admiral Fisher. The sea, however, not agreeing with his delicate health, he returned to school, under the care of the Rev. Mr Spurdens of North Walsham, Norfolk. Having finished his education he visited his uncle, Mr (afterwards Sir) Astley Cooper, who was then rising rapidly in public estimation, and at his suggestion repaired to the Norwich Hospital, where he remained for two years, studying chiefly the practical details of surgery. The Norfolk and Norwich Hospital, to which the writings and practice of Benjamin Gooch had already given considerable celebrity, became still more distinguished under the incumbency of Dr Rigby and Mr Martineau, both already celebrated as expert Lithotomists; and perhaps no provincial hospital could form a more useful school for initiating a young person in the details of surgical practice. This was manifestly one of the principal reasons which led Mr Astley Cooper to advise his nephew to become a pupil at that Institution.

At the end of the two years spent at the Norfolk and Norwich Hospital, Mr Bransby B. Cooper came to London, and by the advice of his uncle, became a house pupil of Mr Joseph Hodgson, then resident in the City, and well known to Mr Astley Cooper as a learned, industrious, and well informed surgeon. Mr Hodgson soon after acquired a high reputation for his elaborate and judicious treatise on the Diseases of Arteries and Veins, published in 1815; and which almost immediately became a classical work on that subject. Under the superintendence of Mr Hodgson, and by his constant attendance at Guy's Hospital, Mr Cooper acquired that minute and accurate acquaintance with Surgical Pathology and surgical practice, which distinguished his subsequent career.

In the year 1812, Mr Cooper entered the army as assistant-surgeon in the Royal Artillery, and immediately repaired to the Peninsula, where our troops were then vigorously engaged. He was present at the battles of Vittoria, the Pyrenees, Nivelle, Orthes, the siege of St Sebastian, and the battle of Toulouse.

Mr Cooper came to Edinburgh in October 1815, and pursued his studies partly at the University, during the seasons 1815–1816 and 1816–1817. On the 10th of November 1815 he became a member of the Royal Medical Society; and during the same winter, 1815–1816, he studied clinical medicine, and became clinical clerk to Dr Rutherford, at the Royal Infirmary. In the winter of 1816–1817 he studied Practical Anatomy at the rooms of Dr John Gordon; attending the lectures of that instructive teacher; and at the same time in 1816, he was elected one of the annual presidents of the Royal Medical Society. It was not the intention of Mr Cooper in studying at Edinburgh, to practise as a physician or to obtain a degree. His object was merely that his education as a surgeon might be more complete and scientific, by being trained to the theoretical principles and practical methods of the Edinburgh School. He was in the habit of saying that he preferred surgical to physical, that is medical cases.

Mr Cooper was admitted a member of the Royal College of Surgeons of England on the 5th of December 1823, having for three years previously acted as Demonstrator of Anatomy at St Thomas's Hospital. In 1843 he was elected an honorary fellow of the College, and in 1848 became a member of the Council.

Mr Bransby Cooper had, at an early period, undertaken the duty of teaching Anatomy by lectures at the Surgical School of Guy's Hospital, and he had devoted himself with great diligence to the practical study of Anatomy. One of the earliest results of his application in this way was the publication in 1825 of his Treatise on the Ligaments, a valuable work, and one much more wanted than is commonly imagined, notwithstanding the elaborate work of Weitbrecht.

In the year 1829 Mr Cooper began the publication of his Lectures on Anatomy, and completed them in four large volumes octavo. These lectures were chiefly intended for the use of students of surgery, and hence they are copiously illustrated by surgical remarks on the applications of anatomical knowledge to diseases and their treatment. Of this work short accounts were given in volume thirty-eight, 1832, and volume thirty-ninth, 1833, of this Journal. It was for the time an instructive and serviceable performance, and deservedly much esteemed by students.

Subsequently he made several valuable contributions in the *Guy's Hospital Reports*, on Surgical Diseases and their treatment; and he communicated two papers to the twen y-seventh volume of the Medico-Chirurgical Transactions.

On the 12th of February 1841 Sir Astley Cooper, after suffering for about nine or ten weeks under symptoms of disease in the heart and aorta, breathed his last. He had left ample materials for publishing a new edition of his large work on Dislocation and Fractures of the Joints; and of this instructive work Mr Bransby Cooper published in 1842 an edition in octavo, greatly enlarged and improved. This was followed before the end of the same year by the Life of Sir Astley Cooper, a work undertaken by Mr Cooper by the desire of his distinguished relative, and composed chiefly from notes left by the subject of the work.

The last considerable performance by Mr Bransby Cooper appeared in October 1851, when he published his Lectures on Surgery, a useful and instructive System of Practical Surgery, which is much esteemed by his pupils and the profession.

When the writer of this notice knew Mr Cooper in Edinburgh, he was very much beloved by his friends and associates. His manner was that of a frank open-hearted good-humoured young man, without pretension, without reserve, and with great natural politeness. In his society it was impossible to believe that you were in company with the nephew of the first surgeon in the metropolis. In 1831, when the writer of this notice again saw him in London, Mr Cooper was surgeon to Guy's Hospital. His manner was the same of the easy, polite, unassuming gentleman, perhaps in a slight degree more quiet, but with the same constant good temper and natural benevolence of manner.

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